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ESSAYS,

POLITICAL, ECONOMICAL,

AND

PHILOSOPHICAL.

Thompson

By BENJAMIN Count of RUMFORD,

KNIGHT OF THE ORDERS OF THE WHITE EAGLE, AND ST. STANISLAUS;
Chamberlain, Privy Counſellor of State, and Lieutenant-General in the Service
of his Moſt Serene Highneſs the ELECTOR PALATINE, *Reigning* DUKE
of BAVARIA; *Colonel of his Regiment of Artillery, and Commander in*
Chief of the General Staff of his Army; F. R. S. Acad. R.
Hiber. Berol. Elec. Boicœ. Palat. et Amer. Soc.

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NO ſubject of philoſophical inquiry, within the limits of human inveſtigation, is more calculated to excite admiration, and to awaken curioſity, than FIRE; and there is certainly none more extenſively uſeful to mankind. It is owing, no doubt, to our being acquainted with it from our infancy,

that

that we are not more ſtruck with its appearance, and more ſenſible of the benefits we derive from it. Almoſt every comfort and convenience which man by his ingenuity procures for himſelf, is obtained by its aſſiſtance; and he is not more diſtinguiſhed from the brute creation by the uſe of ſpeech, than by his power over that wonderful agent.

Having long been accuſtomed to conſider the Management of Heat as a matter of the higheſt importance to mankind, a habit of attending carefully to every circumſtance relative to this intereſting ſubject that occaſionally came under my obſervation, ſoon led me to diſcover how much this ſcience has been neglected, and how much room there is for very eſſential improvements in almoſt all thoſe various operations in which heat is employed for the purpoſes of human life.

The great waſte of Fuel in all countries muſt be apparent to the moſt curſory obſerver; and the uſes to which Fire is employed are ſo very extenſive, and the expence for Fuel makes ſo conſiderable an article in the liſt of neceſſaries, that the importance of the ſubject cannot be denied.

And with regard to the Economy of Fuel, it has this in particular to recommend it, that whatever is ſaved by an individual, is at the ſame time a poſitive ſaving to the whole community; for the leſs demand there is for any article in the market, the lower will be its price; and as all the ſubjects of uſeful induſtry—all the arts and manufactures, without exception, depend, directly or indirectly, on operations in which Fire is neceſſary, it is of much importance to a manufacturing and commercial country to keep the price of Fuel as low as poſſible:—And even in countries where there are no manufactures, and where the inhabitants ſubſiſt entirely by agriculture, if wood be uſed as Fuel—as the proportion of woodland to arable muſt depend in a great meaſure on the conſumption of fire-wood, any ſaving of Fuel will be attended with a proportional diminution of the foreſts reſerved for fire-wood,—conſequently, with an increaſe of the lands under cultivation,—with an increaſe of inhabitants,—and of national wealth, ſtrength, and proſperity.

But what renders this ſubject peculiarly intereſting, is the great relief to the poor in all countries, and particularly in all cold climates, and in all great cities in every climate, that would reſult from any conſiderable diminution of the price of Fuel, or from any ſimple contrivance by which a ſmaller quantity of this neceſſary article than they now are obliged to employ to make themſelves comfortable, might be made to perform the ſame ſervices. Thoſe who have never been expoſed to the inclemencies of the ſeaſons,—who have never been eye-witneſſes to the ſufferings of the poor in their miſerable habitations, pinched with cold and ſtarving with hunger,—can form no idea of the importance *to them* of the ſubject which I propoſe to treat in this Eſſay.

To all thoſe who take pleaſure in doing good to mankind by promoting uſeful knowledge, and facilitating the means of procuring the comforts and conveniencies of life, theſe inveſtigations cannot but be very intereſting.

Though it is generally acknowledged that there is a great waſte of Fuel in all countries, ariſing from ignorance and careleſſneſs in the management of Fire, yet few,—very few, I believe,—are aware of the real amount of this waſte.

From the reſult of all my inquiries upon this ſubject, I have been led to conclude, that not leſs than *ſeven-eighths* of the heat generated, or which, *with proper management*, *might be generated*, from the Fuel actually conſumed, is carried up into the atmoſphere with the ſmoke, and totally loſt. And this opinion has not been formed haſtily; on the contrary, it is the reſult of much attentive obſervation, and of many experiments. But, in a matter of ſo much importance, I feel it to be my duty not merely to give the Public my *opinions*, but to lay before them the grounds upon which thoſe opinions have been founded; in order that every one may judge for himſelf of the certainty, or probability, of my deductions.

It would not be difficult, merely from a conſideration of the nature of heat,—of the manner in which it is generated in the combuſtion of Fuel, and the manner in which it exiſts when generated, —to ſhow that, as the proceſs of boiling is commonly performed, there muſt of neceſſity be a very great loſs of heat; for when the veſſel, in which the fluid to be boiled is contained, is placed over an open or naked fire, not only by far the greater part of the radiant heat is totally loſt, but alſo of that which exiſts in the flame, ſmoke, and hot vapour, a very ſmall proportion only enters the veſſel; the reſt going off with great rapidity, by the chimney, into the higher regions of the atmoſphere. But, without inſiſting upon theſe reaſonings, (though they are certainly incontrovertible,) I ſhall endeavour to eſtabliſh the facts in queſtion upon ſtill more ſolid ground—that of actual experiment.

In the proſecution of the experiments neceſſary in this inveſtigation, I proceeded in the following manner:—As the quantity of heat which any given quantity of any given kind of Fuel is capable of generating is not known, there is no fixed ſtandard with which the reſult of an experiment can be compared, in order to aſcertain exactly the proportion of the heat ſaved, or uſefully employed, to that loſt: Inſtead, therefore, of being able to determine this point *directly*, I was obliged to have recourſe to *approximations*. Inſtead of determining the quantity of heat loſt in any given operation, I endeavoured to find out with how much leſs Fuel the ſame operation might be performed, by a more advantageous arrangement of the Fire, and diſpoſition of the machinery: And ſeveral extenſive public eſtabliſhments, which have been erected in Bavaria within theſe laſt ſix or ſeven years, under my direction, by order of His Moſt Serene Highneſs the ELECTOR PALATINE; particularly an eſtabliſhment for the Poor of Munich (of which an account has been given to the Public in my Firſt Eſſay); and the Eſtabliſhment of a Public Academy for the education of 180 young men, deſtined for the ſervice

of

of the State in the different civil and military departments;—the economical arrangement of theſe eſtabliſhments afforded me a moſt favourable opportunity of putting into practice all my ideas relative to the Management of Fire: and of aſcertaining, by numerous experiments made upon a large ſcale, and often varied and repeated, the real importance of the improvements I have introduced.

That many experiments have been actually made in theſe two eſtabliſhments, during the ſeven years they have exiſted, will not be doubted by thoſe who are informed, that the Kitchen, or rather the Fire-place of the kitchen of the Houſe of Induſtry, has been pulled down and built entirely anew no leſs than *three times*, and that of the Military Academy *twice*, during that period; and that the forms of the boilers, and the internal conſtruction of the fire-places, have been changed ſtill oftener.

The importance of the improvements in the management of heat employed in culinary operations, which have reſulted from theſe inveſtigations, will appear by comparing the quantity of Fuel now actually uſed in thoſe kitchens, to that conſumed in performing the ſame operations in kitchens on the common conſtruction: And this will at the ſame time ſhow, in a clear and ſatisfactory manner, what I propoſed to prove,—namely, that in all the common operations in which Fire is employed, there is a very great waſte of Fuel.

The waſte of Fuel in boiling water or any other liquid over an open fire, in the manner in which that proceſs is commonly performed, and the great ſaving of Fuel which will reſult from a more advantageous diſpoſition and management of the Fire, will be evident from the reſults of the following Experiments, all of which were made by myſelf, and with the utmoſt care.

Experiment, No. 1.

A copper boiler belonging to the kitchen of the Military Academy in Munich, 22 Rhinland inches in diameter above, 19¼ inches in diameter below, and 24 inches in depth, and which weighed 50 lbs. weight of Bavaria, (=61.92 lbs. Avoirdupois,) being fixed in its fire-place, was filled with 95 Bavarian meaſures (= 28 Engliſh wine-gallons) of water, which weighed 187 Bavarian pounds (=232.58 lbs. Avoirdupois); and this water being at the temperature of 58° F. a fire was lighted under the boiler with dry beech-wood, and the water was made to boil, and was continued boiling two hours. The time employed and wood conſumed in this Experiment were as follows:

	Time employed. Hours.	Min.		Wood conſumed. lbs.
To make the water boil, - -	1	1	—	11
To keep the water boiling, - -	2	0	—	2½
Total,	3	1	—	13½

Expe-

Experiment, No. 2.

The ſame boiler, containing the ſame quantity of water at the ſame temperature, being now removed to the kitchen of a private gentleman in the neighbourhood, (Baron de Schwachheim, a brother of the Commandant of the Academy,) and placed upon a tripod, a quantity of the ſame kind of wood uſed in the former Experiment being provided, a fire was lighted under it by the gentleman's cook, (directions having been given to be as ſparing as poſſible of Fuel,) and it was made to boil, and continued boiling two hours.

The reſult of the Experiment was as follows:

	Time employed.		Wood conſumed.
	Hours.	Min.	lbs.
To make the water boil, - -	1	31	45
To keep it boiling, - -	2	0	17½
Total,	3	31	62½

As in theſe two Experiments the ſame boiler was employed;—as the quantity of water was the ſame,—as alſo its temperature at the beginning of the Experiments,—and as it was made to continue boiling during the ſame length of time, it is evident that the quantities of wood conſumed ſhow the relative advantages of the different methods employed in the management of the Fire. The difference of theſe quantities of Fuel is very great (the one being only 13½ lbs. and the other amounting to no leſs than 62½ lbs.) And this ſhows how very conſiderable the waſte of Fuel really is, in the manner in which it is commonly employed for culinary purpoſes, and how important the ſavings are which may be made by introducing a more advantageous arrangement for the management of Fire. But, great as theſe ſavings may appear to be, as ſhown by the reſults of the foregoing Experiments, yet they are in fact ſtill more conſiderable, as will be abundantly proved in the ſequel. In the Experiment, No. 2. in which the boiler was put over an open Fire, great care was taken to place the Fuel in the moſt advantageous manner; but, in general, little attention is paid to that circumſtance, and the waſte of Fuel is greatly increaſed by ſuch negligence: But in cloſed fire-places, upon a good conſtruction, as the *proper place* for the Fuel cannot be miſtaken, and as it is fixed and bounded on all ſides by a wall, the ignorance or inattention of thoſe who take care of the Fire can never be productive of any great waſte of Fuel; and this is an advantage of no ſmall importance attending theſe fire-places.

Experiment, No. 3.

A large copper ſauce-pan or *caſſerole*, 11¾ inches in diameter above,—10¾ in diameter below, and 3¾ inches deep, containing 4 meaſures of water, weighing $7\frac{15}{16}$ lbs. and at the temperature of 58° F. being placed in its cloſed fire-place, and a fire being made under it with ſmall pieces of dry beech-wood cut in lengths of about 4 inches, the water was made to boil, and was continued boiling two hours.

The reſult of the Experiment was as follows:

	Time employed.			Wood conſumed.
	Hours.	Min.		lbs.
To make the water boil, - -	0	12	—	1
To keep it boiling, - -	2	0	—	0¾
Total,	2	12	—	1¾

Experiment, No. 4.

The ſame ſauce-pan, containing the ſame quantity of water, and at the ſame temperature as in the laſt Experiment, was now taken from its proper fire-place, and placed upon a tripod; and a fire being made under it with dry beech-wood, the reſult of the Experiment was as follows:

	Time employed.			Wood conſumed.
	Hours.	Min.		lbs.
To make the water boil, - -	0	28	—	6
To keep it boiling, - -	2	0	—	5½
Total,	2	28	—	11½

The difference in the reſult of theſe two Experiments is nearly the ſame as that in the reſults of thoſe before mentioned, and they all tend to ſhow, that in cooking, or boiling over an open fire, nearly *five times* as much Fuel is required, as when the heat is confined in a cloſed fire-place, and its operation properly directed.

But I muſt again repeat, what I have already obſerved with reſpect to the two former Experiments, as the Experiments No. 2. and No. 4. were both made with the utmoſt care, the reſults of them, compared with thoſe which were made with the ſame boilers placed in cloſed fire-places, can give no adequate idea of the real loſs of heat, and waſte of Fuel, which take place in the common operations of cookery.

From ſeveral eſtimates which I have made with great care relative to this ſubject, founded upon the quantity of Fuel actually conſumed in the kitchens of ſeveral private families, compared with the quantities of different kinds of food prepared for the table, it appears that at leaſt *nine-tenths* of the wood actually conſumed in common kitchens, where cooking is carried on over an open fire, might be ſaved, by introducing the various improvements I have brought into uſe in the kitchens that have been conſtructed under my directions.

But it is not alone in kitchens, in which cooking is carried on over open fires, that uſeful alterations may be made; kitchens with cloſed fire-places, and indeed all the kitchens that have yet been contrived,

trived, (as far as my knowledge extends,) are ſuſceptible of great improvement.

The various improvements that may be made in mechanical arrangements for the Economy of Fuel, will appear in a ſtriking manner from a detail of the different alterations which have from time to time been made in the kitchen of the Houſe of Induſtry at Munich, and in that of the Military Academy, and of the effects produced by thoſe progreſſive improvements.

The Houſe of Induſtry being an eſtabliſhment of public charity, and the number of thoſe fed from the kitchen amounting from 1000 to 1500 perſons daily, the Economy of Fuel, in a kitchen upon ſo large a ſcale, became an object of ſerious conſideration; and I attended to this matter with peculiar pleaſure, as it ſo completely coincided with my favourite philoſophical purſuits.

The inveſtigation of Heat, and of the laws of its operations, had long occupied my attention, and I had been ſo fortunate, in the courſe of my Experiments upon that ſubject, as to make ſome diſcoveries which were thought worthy of being inſerted in the Philoſophical Tranſactions of the Royal Society of London; and, for my laſt paper upon that ſubject, publiſhed in the Tranſactions for the year 1792, I had the honour to receive the annual Medal of the Society. I hope my mentioning this circumſtance will not be attributed to oſtentation. My motive in doing it, is merely to ſhow, that when I undertook to make the arrangements of which I am about to give an account, the ſubject was by no means new to me; but, on the contrary, that I was prepared, and in ſome meaſure qualified, for ſuch inveſtigation.

I conceive it to be the duty of thoſe who propoſe uſeful improvements for the benefit of mankind, not only to *merit*, but alſo to do every thing in their power to *obtain*, the confidence of thoſe to whom their propoſals are ſubmitted; and there appears to me to be a much greater degree of pride and arrogance diſplayed by an author *in taking it for granted* that the world is already ſufficiently acquainted with his merit and his qualifications to treat the ſubject he undertakes to inveſtigate, than in modeſtly pointing out the grounds upon which the confidence of the Public in his knowledge of his ſubject, and in his integrity, may be founded.

But to return from this digreſſion. In the firſt arrangement of the kitchen in the Houſe of Induſtry at Munich, which was finiſhed in the beginning of the year 1790, eight large copper boilers, each capable of containing about 38 Engliſh wine gallons, were placed in ſuch a manner in two rows, in a ſolid maſs of brick-work, 3 feet high, 9 feet wide, and 18 feet long, built in the middle of the kitchen, that, from a ſingle fire-place, ſituated at one end of this brick-work, by means of canals (furniſhed with valves or dampers) going from it, through the ſolid maſs of the brick-work to all the different boilers, theſe boilers were all heated, and made to boil with one ſingle fire; and though none of them were in actual contact with the fire-place, and ſome of them were diſtant from it near 15 feet, yet they were all heated with great facility, and in a ſhort ſpace of time, by the heat which, upon opening the valves, (which were of iron,) was made to paſs through the canals.

Each boiler having its ſeparate canal, and its ſeparate valves, any ſingle boiler, or any number of them, might be heated at pleaſure, without heating the reſt; and by opening the valves of any boiler more or leſs, more or leſs heat, as the occaſion required, might be made to paſs under the boiler;—and when no more heat was wanting for any of the boilers, or when the fire was too ſtrong, by opening a particular valve, a communication with a *waſte canal* was formed, by which all the heat, or any part of it, at pleaſure, might be made to paſs off directly into the chimney, without going near any of the boilers.

The Fire was regulated by a regiſter in the door of the aſh-pit, by which the air was admitted into the fire-place; and, when no more heat was wanted, the Fire was put out by cloſing this regiſter entirely, and by cloſing at the ſame time all the valves or dampers in the canals leading from the fire-place.

The fire-place was of an oval form, 3 feet long, 2 feet 3 inches wide, and about 18 inches high, vaulted above with *a double vault*, 4 inches of air being left between the two vaults; and the Fuel was introduced into the fire-place by a paſſage cloſed by a *double* iron door, which door was kept conſtantly ſhut;—and the Fuel was burnt upon an iron grate; the air which ſupplied the Fire coming up from below the grate through the aſh-pit.

The loſs of heat in its paſſage from the fire-place to the boilers, was prevented by making the canals of communication *double*, one within the other; the internal canal by which the heat paſſed, and which was 5 inches wide internally, and 6 inches high, being itſelf placed, and, as it were, *inſulated*, in a canal ſtill larger, in ſuch a manner that the canal by which the heat paſſed, (which was conſtructed of very thin bricks, or rather tiles,) was *ſurrounded on every ſide* with a wall, 2 inches thick, of *confined air*. The ſurrounding canal being formed in the ſolid body of the maſs of brick-work, this arrangement of the double canals was entirely concealed. The double canals and the double vault over the fire-place were intended to ſerve the ſame purpoſe, namely, *to confine more effectually the heat*, and prevent its eſcape into the maſs of brick-work, and its conſequent loſs.

Having found, in the courſe of my experiments, that confined air is the beſt barrier * that can be oppoſed to heat, to confine it, I endeavoured to avail myſelf of that diſcovery in theſe economical arrangements, and my attempts were not unſucceſsful.

Not only the fire-place itſelf, and the canals of communication between the fire-place and the boilers, were ſurrounded by confined air, but it was alſo made uſe of for confining the heat in the boilers, and preventing its eſcaping into the atmoſphere. This was done by making the covers of the boilers *double.* Theſe covers, (See the figures 1 and 2, Plate I.) which were made of tin, or rather of thin iron-plates tinned, were in the form of a hollow cone; the height of the cone was equal to about one-third of its diameter; and the air which it contained was entirely ſhut up, the bottom of the cone being cloſed by a circular plate or thin ſheet of tinned iron. The bottom of the cone was accurately fitted to the top of the boiler, which it completely cloſed by means of a rim about 2 inches wide, which entered the boiler; which rim was ſoldered to the flat ſheet of tinned iron which formed the bottom of the cover. The ſteam, generated by the boiling liquid, was carried off by a tube about half an inch in diameter, which paſſed through the hollow conical cover, and which was attached to the cover, both above and below, with ſolder, in ſuch a manner that the air with which the hollow cone was filled, remained completely confined, and cut off from all communication with the external air of the atmoſphere, as well as with the ſteam it generated in the boiler.

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* See Philoſophical Tranſactions, 1792, Part I.

In ſome of the covers I filled the hollow of the cone with fur; but I did not find that theſe were ſenſibly better for confining the heat than thoſe in which the cone was filled ſimply with air.

To convince the numerous ſtrangers, who from curioſity viſited this kitchen, of the great advantage of making uſe of double covers to confine the heat in the boilers, inſtead of uſing ſingle covers for that purpoſe, a ſingle cover was provided, which, as it was externally of the ſame form as the others, when it was placed upon a boiler, could not be diſtinguiſhed from them; but as its bottom was wanting, and conſequently there was no confined air interpoſed between the hot ſteam in the boiler and the external ſurface of the cover, on being placed upon a kettle actually boiling, this cover inſtantaneouſly became ſo exceedingly hot as actually to burn thoſe who ventured to touch it;—while a *double cover*, formed of the ſame materials, and placed in the ſame ſituation, was ſo moderately warm that the naked hand might be held upon it for any length of time without the leaſt inconvenience.

As it was eaſy to conceive that what was ſo exceedingly hot as to burn the hand, in an inſtant, upon touching it, could not fail to communicate a great deal of heat to the cold atmoſphere, which continually lay upon it, this Experiment ſhowed, in a ſtriking and *convincing* manner, the utility of my double covers; and I have ſince had the ſatiſfaction to ſee them gradually finding their way into common uſe.

It is perhaps quite unneceſſary that I ſhould inform my readers, that one principal motive which induced me to take ſo much pains in the arrangement of this kitchen, was a deſire to introduce uſeful improvements relative to the Management of Heat and the Economy of Fuel, into common practice. An eſtabliſhment ſo intereſting in all reſpects,—ſo important in its conſequences,—and ſo perfectly new in Bavaria, as a public Houſe of Induſtry upon a liberal and extenſive plan,—where almoſt every trade and manufacture is carried on under the ſame roof,—where the poor and indigent of both ſexes, and of all ages, find a comfortable aſylum, and employment ſuited to their ſtrength and to their talents; and where induſtry is excited, *not by puniſhments*, but by *the moſt liberal rewards*, and by the kindeſt uſage: Such an eſtabliſhment, I thought, could not fail to excite the curioſity of the Public, and to draw together a great concourſe of viſitors; and as this appeared to me a favourable opportunity to draw the public attention to uſeful improvements, all my meaſures were taken accordingly; and not only the kitchen, but alſo the bakehouſe,—the ſtoves for heating the rooms,—the lamps,—the various utenſils and machines made uſe of in the different manufactories,—all the different economical arrangements and contrivances for facilitating the operations of uſeful induſtry, were ſo many models expreſsly made for imitation.

But in the arrangements relative to the Economy of Fuel, beſides a view to immediate public utility, another motive, not much leſs powerful, contributed to induce me to pay all poſſible attention to the ſubject; namely, a deſire to acquire a more thorough knowledge relative to the nature of Heat, and of the laws of its operations; and with this view ſeveral parts were added to the machinery, which I ſuſpected at the time to be too complicated to be really uſeful in common practice.

The ſteam, for inſtance, which aroſe from the boiling liquids, inſtead of being ſuffered to eſcape into the atmoſphere, was carried up by tubes into a room immediately over the kitchen, where it was made to paſs through a ſpiral worm, placed in a large caſk full of cold water, and condenſed, giving out its heat to the water in the caſk; which water thus warmed, without any new expence of Fuel, was made uſe of the next day, inſtead of cold water, for filling the boilers. That this water, ſo warmed, might not be cooled during the night, the caſk that contained it was put into another caſk ſtill larger; and the ſpace between the two caſks was filled with wool. The cooling of the ſteam, in its paſſage from the boiler to the caſk where it was condenſed, was prevented by warm coverings of ſheep-ſkins with the wool on them, by which the tubes of communication, which were of tin, were defended from the cold air of the atmoſphere.

By this contrivance, the heat, that would otherwiſe have been carried off by the ſteam into the atmoſphere and totally loſt, was arreſted in its flight, and brought back into the boiler, and made to work the ſecond day.

By other contrivances, the ſmoke alſo was laid under contribution. After it had paſſed under the

boilers,

boilers, and juſt as it was about to eſcape by the chimney, it was ſtopped; and, by being made to paſs under a large copper filled with cold water, was deprived of the greater part of the heat it ſtill retained: And thinking it probable that conſiderable advantages would be derived from drying the wood very thoroughly, and even heating it, before it was made uſe of for Fuel, the ſmoke from two of the boilers was made to paſs under a plate of iron which formed the bottom of an oven, in which the wood, neceſſary for the conſumption of the kitchen for one day, (having been previouſly cut into billets of a proper ſize,) was dried during 24 hours, previous to its being uſed.

In a ſmaller kitchen, (adjoining to that I have been deſcribing,) which was conſtructed merely as a model for imitation, and which was conſtantly open for the inſpection of the Public, five boilers of different ſizes, all heated by the ſame fire, were placed in a ſemicircular maſs of brick-work, and the ſmoke, after having paſſed under all theſe five boilers, was made to heat, at pleaſure, either an oven, or water which was contained in a wooden caſk ſet upright upon the brick-work.—A tube of copper, tinned on the outſide, which went through the caſk, gave a paſſage to the ſmoke, and this tube was connected with the bottom of the caſk by means of a circular plate of copper through which the tube paſſed, which plate cloſed a circular opening in the bottom of the caſk ſomewhat larger in diameter than the tube.

This circular plate was nailed to the bottom of the caſk, and the joining made water-tight by interpoſing between the metallic plate and the wood a ſheet of paſteboard; and the tube was faſtened to the plate with ſolder. This tube, (which was about 6 inches in diameter,) as ſoon as it had paſſed the circular plate, and entered the barrel, branched out into three ſmaller tubes, each about 4 inches in diameter, which, running parallel to each other through the whole length of the caſk, went out of it above, by three different holes in the upper head of the caſk, and ended in a canal which led to the chimney.

This tube, by which the ſmoke paſſed through the caſk, was branched out into a number of branches in order to increaſe the ſurface, by which the heat of the ſmoke was communicated to the water in the caſk. The caſk was ſupplied with water from a reſervoir, placed in the upper part of the building, by means of a leaden pipe of communication from the one to the other; and the machinery was ſo contrived, that, when any water was drawn out of the caſk for uſe, it was immediately replaced from the reſervoir; but as ſoon as the water in the caſk had regained its proper height, the cold water from the reſervoir ceaſed to flow in it.

Nothing more generally excited the ſurpriſe and curioſity of thoſe who viſited this kitchen, than to ſee water actually boiled in a wooden caſk, and drawn from it boiling hot, by a braſs cock. I have been the more particular in deſcribing the manner in which this was done, as I have reaſon to think that a contrivance of this kind, or ſomething ſimilar to it, might, in many caſes, be applied to uſeful purpoſes. No contrivance can poſſibly be invented by which heat can be communicated to fluids with ſo little loſs; and as wood is not only an excellent non-conductor of heat itſelf, but may eaſily be ſurrounded by confined air, by furs and other like bodies which are known to be uſeful in confining heat, the loſs of heat, by the ſides of a containing veſſel compoſed of wood, might be almoſt entirely prevented.

Why ſhould not the boilers for large ſalt-works and breweries, and thoſe deſtined for other ſimilar proceſſes, in which great quantities of water are heated, or evaporated, be conſtructed of wood, with horizontal tubes of iron or copper, communicating with the fire-place, and running through them, for the circulation of the ſmoke?—But this is not the place to enlarge upon this ſubject; I ſhall therefore leave it for the preſent, and return to my kitchens.

To prepare the ſoup furniſhed to the Poor from the kitchen of the Houſe of Induſtry, it was found neceſſary to keep up the fire near *five hours*, the ſoup, in order to its being good, requiring to be kept actually boiling above three hours.

The Fuel made uſe of in this kitchen was dry beech-wood; a cord of which, (or *klafter*, as it is called,) 5 Engliſh feet $8\frac{9}{10}$ inches long, 5 feet $8\frac{9}{10}$ inches high, and 3 feet $1\frac{1}{3}$ inches wide, and which weighed at an average about 2200 Bavarian pounds, 2 (=724 lbs. Avoirdupois,) coſt at an average about 5¼ florins (= 9s. 6½ d. ſterling) in the market.

Of this wood the daily conſumption, when ſoup was provided for 1000 perſons, was about 300 lbs. Bavarian weight, or about 1/7, or more exactly 3/22 of a cord or klafter, which coſt 43 creutzers, (60 creutzers making a florin,) or about 1 s. 3½ d. ſterling: And this gives 1/23 of a creutzer, or 1/20 of a farthing, for the daily expence for Fuel in cooking for each perſon.

To make an eſtimate of the daily expence for Fuel in cooking the ſame quantity of the ſame kind of ſoup in private kitchens, we will ſuppoſe theſe 1000 perſons, who were fed from the public kitchen of the Houſe of Induſtry, to be ſeparated into families of 5 perſons each.

This would make juſt 200 families; and the quantity of wood conſumed in the public kitchen daily for feeding 1000 perſons, (= 300 lbs.,) being divided among 200 families, gives 1½ lbs. of wood for the daily conſumption of each family; and according to this eſtimate, 1 cord of wood, weighing 2200 lbs. ought to ſuffice for cooking for ſuch a family 1466 days, or 4 years and 6 days.

But upon the moſt careful inquiries relative to the real conſumption of Fuel in private families in operations of cookery, as they are now generally performed over an open fire, I find that 5 Bavarian pounds of good peas-ſoup can hardly be prepared at a leſs expence of Fuel than 15 lbs. of dry beech-wood of the beſt quality; conſequently, a cord of ſuch wood, inſtead of ſufficing for preparing a ſoup daily for a family of 5 perſons for 4 years, would hardly ſuffice for ſo long a time as 5 months.

And hence it appears, that the conſumption of Fuel in the kitchens of private families, is to that conſumed in the firſt kitchen of the Houſe of Induſtry at Munich, *in preparing the ſame quantity of the ſame kind of food*, (peas-ſoup,) as 10 to 1 *. But it muſt be remembered, that this difference in the quantities of Fuel expended is not occaſioned *entirely* by the difference between the two methods of managing the Fire; for, excluſive of the effect produced by a given arrangement of the machinery, —with the ſame arrangement, the greater the quantity of food prepared at once, or the larger the boiler, (within certain limits however, as will be ſeen hereafter,) the leſs in proportion will be the quantity of Fuel required;—and the ſaving of Fuel which ariſes from cooking upon a large ſcale is very conſiderable. But I ſhall take occaſion to treat this part of my ſubject more fully elſewhere.

The kitchen in the Houſe of Induſtry was finiſhed in the beginning of the year 1790. And much about the ſame time, two other public kitchens upon a large ſcale were erected at Munich, under my direction; namely, the kitchen belonging to the Military Academy, and that belonging to the Military Hall (as it is called) in the Engliſh garden, in which building near 200 military officers meſſed daily during the annual encampments,—for which purpoſe this building was erected.

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* Afterwards, on altering the kitchen of the Houſe of Induſtry, and fitting it up on better principles, the Economy of Fuel was carried ſtill farther, as will be ſeen in the ſequel of this Eſſay.

There is likewiſe in the garden, (which is 6 Engliſh miles in circumference,) an inn—a farm-houſe, and a large dairy; and theſe eſtabliſhments gave me an opportunity of conſtructing no leſs than four other kitchens;—namely, two for the inn, one for the farm-houſe, and one for the uſe of the dairy. And the uſes for which theſe different kitchens were deſigned, and to which they were applied, were ſo various, as not only to include almoſt every proceſs of cookery, but alſo to afford opportunities of performing the ſame operations upon very different ſcales, and conſequently of making many intereſting Experiments relative to the Management of heat, and the Economy of Fuel.

That I did not neglect theſe opportunities of purſuing, with effect, a ſubject which had long engaged my attention, and to which I was much attached, will readily be believed by thoſe who know what ardour a curious ſubject of philoſophical inveſtigation is capable of inſpiring in an inquiſitive mind.

As the Experiments I have made, or cauſed to be made, in the different eſtabliſhments before mentioned, during the ſix or ſeven years that they have exiſted, are extremely numerous; it would take up too much time to give an account of them in detail; I ſhall therefore content myſelf with merely noticing the general reſults of them, and mentioning more particularly only ſuch of them as appear to me to be moſt important. And in regard to the peculiar conſtruction of the different kitchens above mentioned, as moſt of them have undergone many alterations, and as no one of them remains exactly in the ſame ſtate in which it was firſt conſtructed, I do not think it neceſſary to be very particular in my account of them; I ſhall occaſionally mention the principles on which they were conſtructed, and the faults I diſcovered in them; but when I ſhall come to ſpeak of thoſe improvements which have ſtood the teſt of actual experience, and which I can recommend as being worthy of imitation, I ſhall take care to be very exact and particular in my deſcriptions.

It will not be found very difficult, I fancy, from what has been ſaid, to form a pretty juſt idea of the conſtruction of the kitchen in the Houſe of Induſtry above deſcribed, even without the help of a plan or drawing of it. That in the Military Academy was conſtructed upon a different principle: Inſtead of heating all the boilers from one and the ſame fire-place, almoſt every boiler had its own ſeparate fire-place; and though the boilers were all furniſhed with double covers, ſimilar to thoſe made uſe of in the kitchen of the Houſe of Induſtry, yet there was no attempt made to recover the heat carried off by the ſteam, but it was ſuffered to eſcape without hindrance into the atmoſphere; it having been found, by the experiments made in the kitchen of the

Houſe of Induſtry, that when the Fire is properly managed, that is to ſay, when the heat is but juſt ſufficient to keep the liquid boiling hot, or *very gently boiling*, the quantity of ſteam generated is inconſiderable, and the heat carried off by it not worth the trouble of ſaving.

Each fire-place was furniſhed with an iron grate, upon which the wood was burnt, and the opening into the fire, as well as that which communicated with the aſh-pit, had in each its ſeparate iron door.

Finding afterwards that the iron door which cloſed the opening by which the wood was introduced into the fire-place, was much heated, and conſequently that it cauſed a conſiderable loſs of heat by communicating it to the cold atmoſphere with which it was in contact; in order to remedy this evil without incurring the expence of double doors, the iron door was removed, and in its ſtead was placed a hollow cylinder, or rather truncated cone, of burnt clay or common earthen-ware, which cone was 4 inches long, 6 inches in diameter internally, and 8 inches in diameter externally, at its larger end or baſe; and 5½ inches in diameter internally, and 7½ inches in diameter externally, at its ſmaller end: And being firmly fixed, with its axis in an horizontal poſition, and its larger end or baſe outwards, in the middle of the opening leading to the fire-place, and being well united with the ſolid brick-work by means of mortar, the cavity of this cone formed the opening by which the wood was introduced into the fire-place. This cavity being cloſed with a fit ſtopper of earthen-ware, as earthen-ware is a non-conductor of heat, or as heat cannot paſs through it but with great difficulty, and very ſlowly, the external ſurface of this cone and its ſtopper were never much heated, conſequently the quantity of heat they could communicate to the atmoſphere was but very trifling. This contrivance was afterwards rendered much more ſimple, by ſubſtituting, inſtead of the hollow cone, a tile, 10 inches ſquare, and about 2½ inches thick, with a conical hole in its center, 6 inches in diameter externally, and 5¼ inches in diameter within, provided with a fit baked earthen ſtopper. (See the Figures, N° 6, 7, and 8. Plate I.)

A perforated ſquare tile is preferable to a hollow cylinder for forming a paſſage into the fire-place, not only becauſe it is cheaper, ſtronger, and more durable, but alſo becauſe it may, on account of its form, be more eaſily and more firmly fixed in its place, and united with the reſt of the brick-work.

If proper moulds be provided for forming theſe perforated tiles and their ſtoppers, they may be afforded for a mere trifle. In Munich they are made of the very beſt earth, by the Elector's potter, and they coſt no more than 24 creutzers, or ſomething leſs than 9 d. ſterling, for a tile with its ſtopper. I had ſeveral made of ſand-ſtone by a ſtone-cutter, but they coſt me 1 florin and 30 creutzers, or about 2 s. 9 d. ſterling each.

Though thoſe made of ſtone anſwered perfectly well, yet I found them not better than thoſe made of earthen-ware; and as theſe laſt are much cheaper, and I believe equally durable, they ought certainly to be preferred. That the ſtopper may be made to fit with accuracy the hole it is intended to cloſe, (which is neceſſary, as will be ſeen hereafter,) they may be ground together with fine ſand moiſtened with water.

Senſible, from the beginning, of the great importance of being abſolutely maſter of the air that is admitted into the fire-place to feed the Fire, ſo as to be able to admit more or leſs at pleaſure, or to exclude it entirely; I took care, in all my fire-places, to cloſe very exactly the paſſage into the aſh-pit by a door carefully fitted to its frame, the air being admitted through a ſemicircular opening furniſhed with a regiſter in the middle of this door. This contrivance (which admits of no further improvement) is indiſpenſably neceſſary in all well-conſtructed fire-places, great or ſmall. (See the Figures from Fig. 9 to Fig. 16. Plate II.)

Having occaſion, in the courſe of my arrangements, to make uſe of a great number of boilers, and often of ſeveral boilers of the ſame dimenſions, I availed myſelf of that circumſtance to determine, by actual experiments, the beſt form for boilers, or that form which, with any given capacity, ſhall be beſt adapted for ſaving Fuel.

Two or more boilers of the ſame capacity, but of different forms, conſtructed of ſheet copper of the ſame thickneſs, were placed in cloſed fire-places, conſtructed as nearly as poſſible upon the ſame principles, and were uſed for a length of time in the ſame culinary proceſſes; and the quantity of Fuel conſumed by each being noted, the comparative advantages of their different forms were aſcertained. Some of theſe boilers were made deep and narrow;—others wide and ſhallow;—there were ſome with flat bottoms; others of a globular form; and others again with their bottoms drawn inward like the bottom of a common glaſs bottle. The reſults of theſe inquiries were very curious, and led me to a moſt intereſting diſcovery:—They taught me not only what forms are beſt for boilers; but alſo (what is ſtill more intereſting) *why* one form is preferable to another:—They gave me much new light with reſpect to the *manner* in which flame and hot vapour part with their heat; and ſuggeſted to me the idea of a very important improvement in the internal conſtruction of fire-places, which I have ſince put in practice with great ſucceſs.

But in order to be able to explain this matter in a clear and ſatisfactory manner, and to render it eaſier to be underſtood by thoſe who have not been much converſant in inquiries of this kind, it will be neceſſary to go back a little, and to treat the ſubject under conſideration in a more regular and ſcientific manner.

Though it was not my intention originally to write an elementary treatiſe on Heat, yet, as the firſt or fundamental principles of that ſcience are neceſſary to be known, in order to eſtabliſh upon ſolid grounds the practical rules and directions relative to the Management of Heat which will hereafter be recommended, it will not, I truſt, be deemed either improper or ſuperfluous, to take a more extenſive view of the ſubject, and to treat it methodically, and at ſome length.

I have perhaps already expoſed myſelf to criticiſm, by paying ſo little attention to method in this Eſſay, as to poſtpone ſo long the inveſtigation of the elementary principles of the ſcience I have undertaken to treat.—It may be thought that the part of the ſubject I am now about to conſider ſhould have preceded all other inveſtigation;—that inſtead of occupying the middle of my book, it ought to have been diſcuſſed in the Introduction, or at leaſt to have been treated in the beginning of the firſt chapter:—But if I have been guilty of a fault in the arrangement of my ſubject, it has ariſen not from inattention, but from an error of judgment. Deſirous rather of writing an *uſeful book*, than of being the Author of a *ſplendid performance*, I have not ſcrupled to tranſgreſs the eſtabliſhed rules of elegant compoſition in all caſes where I thought it would contribute to my main deſign, *public utility*:—And well aware that my book, in order to its being really uſeful, muſt be read by many who have neither time nor patience to labour through an elementary treatiſe upon ſo abſtruſe a ſubject, I have endeavoured to *decoy* my reader into the ſituation in which I wiſh him to be placed, in order to his having a complete view of the proſpect I have prepared for him, rather than to force him into it. If I have uſed art in doing this, he muſt forgive me; my deſign was not only innocent, but ſuch as ought to entitle me to his thanks and to his eſteem. I wiſhed to entice him on as far as poſſible, without letting him perceive the difficulties of the road; and now that we have come on together ſo far, and are ſo near our journey's end, I hope and truſt that he will not leave me.—To proceed therefore—

CHAP. II.

Of the GENERATION OF HEAT *in the* COMBUSTION OF FUEL.—*Without knowing what Heat really is, the Laws of its Action may be inveſtigated.—Probability that the Heat generated in the Combuſtion of Fuel is furniſhed by the Air, and not by the Fuel.—Effects of blowing a Fire explained.—Of Fire-places in which the Fire is made to blow itſelf.—Of Air-furnaces.—Theſe Fire-places illuſtrated by a Lamp on* Argand'*s Principle.—Great Importance of being able to regulate the Quantity of Air which enters a cloſed Fire-place.—Utility of Dampers in the Chimnies of cloſed Fire-places.—General Rules and Directions for conſtructing cloſed Fire-places; with a full Explanation of the Principles on which theſe Rules are founded.*

WITHOUT entering into thoſe abſtruſe and moſt difficult inveſtigations reſpecting the Nature of FIRE, which have employed the attention and divided the opinions of ſpeculative philoſophers in all ages;—without even attempting to determine whether there be ſuch a thing as an *igneous* fluid, or not;—whether what we call *Heat* be occaſioned by the accumulation, or by the increaſed action of ſuch a fluid;—or whether it ariſes merely from an increaſed motion in the component particles of the body heated, or of ſome elaſtic fluid by which thoſe particles are ſuppoſed to be ſurrounded, and upon which they are ſuppoſed to act, or by which they are ſuppoſed to be acted upon:—In ſhort, without bewildering myſelf and my reader in this endleſs labyrinth of darkneſs and uncertainty, I ſhall confine my inquiries to objects more uſeful, and which are clearly within the reach of human inveſtigation;—namely, the diſcovery of the ſenſible properties of Heat, and of the moſt advantageous methods of generating it, and of directing it with certainty and effect in thoſe various proceſſes in which it is employed in the economy of human life.

Though I do not undertake to determine *what Heat really is*, nor even to offer any opinions or conjectures relative to that ſubject; yet as Heat is evidently ſomething capable of being excited or generated, increaſed or accumulated, meaſured and transferred from one body to another; in treating the ſubject, I ſhall ſpeak of it as being *generated*, *confined*, *directed*, *diſperſed*, &c.; it being neceſſary to uſe theſe terms in order to make myſelf underſtood.

Though it is not known exactly *how much* Heat it is poſſible to produce in the combuſtion of any given quantity of any given kind of Fuel, yet it is more than probable that the quantity depends in a great meaſure on the Management of the Fire. It is likewiſe probable—I might ſay, certain—that the Heat produced is furniſhed, not merely by the

Fuel, but in a great meaſure, if not entirely, by the *air* by which the Fire is fed and ſupported. It is well known that air is neceſſary to combuſtion; it is likewiſe known that the pure part of common atmoſpheric air, or that part of it (amounting to about $\frac{1}{4}$ of its whole volume) which alone is capable of ſupporting the combuſtion of inflammable bodies, undergoes a remarkable change, or is actually *decompoſed* in that proceſs; and as in this decompoſition of pure air a great quantity of heat is known to be ſet looſe, or to become redundant, it has been ſuppoſed by many, (and with much appearance of probability,) that by far the greater part, if not all the heat produced in the combuſtion of inflammable bodies, is derived from this ſource.

But whether it be the air, or the Fuel, which furniſhes the heat, it ſeems to be quite certain that the quantity furniſhed depends much upon *the Management of the Fire*, and that the quantity is greater as the combuſtion or decompoſition of the Fuel is more complete. In all probability, the decompoſition of the air keeps pace with the decompoſition of the Fuel.

It is well known that the conſumption of Fuel is much accelerated, and the intenſity of the heat augmented, by cauſing the air by which the combuſtion is excited, to flow into the fire-place in a continued ſtream, and with a certain degree of velocity. Hence, blowing a fire, when the current of air is properly directed, and when it is not too ſtrong, ſerves to accelerate the combuſtion, and to increaſe the heat; but when the blaſt is improperly directed, it will rather ſerve to derange and to impede the combuſtion than to forward it; and when it is too ſtrong it will blow the Fire quite out, or totally extinguiſh it. There is no Fire, however intenſe, but may be blown out by a blaſt of air, provided it be ſufficiently ſtrong, and that as infallibly as by a ſtream of cold water. Even gun-powder, the moſt inflammable perhaps of known ſubſtances, may be actually on fire at its ſurface, and yet the Fire may be blown out and extinguiſhed before the grain of powder has had time to be entirely conſumed.

This fact, however extraordinary and incredible it may appear, I have proved by the moſt unexceptionable and concluſive experiments *.

Fire-places may be ſo conſtructed that the Fire may be made to blow itſelf, or—which is the ſame thing—to cauſe a current of air to flow into the Fire: And this is an object to which the greateſt attention ought to be paid in the conſtruction of all fire-places where it is not intended to make uſe of an artificial blaſt from bellows for blowing the Fire. Furnances conſtructed upon this principle have been called *air-furnaces*; but every fire-place, and particulary every cloſed fire-place, ought to be an air-furnace, and that even were it intended to ſerve only for the ſmalleſt ſauce-pan, otherwiſe it cannot be perfect.

An Argand's lamp is a fire-place upon this conſtruction; for the glaſs tube which ſurrounds the wick (and which diſtinguiſhes this lamp from all others) ſerves merely as a blower. The circular form of the wick is not eſſential; for by applying a flatted glaſs tube as a blower to a lamp with a flat or riband wick, it may be made to give as much light as an Argand's lamp; or at leaſt quite as much in proportion to the ſize of the wick; and to the quantity of oil conſumed, as I have found by actual experiment.

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* See Philoſophical Tranſactions for the year 1797—Part II. Page 282.

But it is not the light alone that is increaſed in conſequence of the application of theſe blowers;—the heat alſo is rendered much more intenſe;—and as the heat of any fire may be increaſed by a ſimilar contrivance, on that account it is that I have had recourſe to theſe lamps to aſſiſt me in explaining the ſubject under conſideration. In theſe lamps the fire-place is cloſed on all ſides, and the current of air which feeds the Fire riſes up perpendicularly from below the fire-place into the Fire. By ſurrounding the Fire on all ſides by a wall, the cold atmoſphere is prevented from ruſhing in laterally from all quarters to ſupply the place of the heated air or vapour, which, in conſequence of its increaſed elaſticity from the heat, continually riſes from the Fire, and this cauſes the current of air below (the only quarter from which it can with advantage flow into the Fire) to be very ſtrong.

But in order that a fire-place may be perfect, it ſhould be ſo contrived that the combuſtion of the Fuel, and the generation of the heat, may occaſionally be accelerated or retarded, *without adding to or diminiſhing the quantity of Fuel;* and, when the fire-place is cloſed, this may eaſily be done by means of a *regiſter* in the door which cloſes the paſſage leading to the aſh-pit;—for, as the rapidity of the combuſtion depends upon the quantity of air by which the Fire is fed, by opening the regiſter more or leſs, more or leſs air will be admitted into the fire-place, and conſequently more or leſs Fuel will be conſumed, and more or leſs heat generated in any given time, though the quantity of Fuel in the fire-place be actually much greater than what otherwiſe would be ſufficient.—Fig. 9. ſhews the form of the regiſter I commonly uſe for this purpoſe.

In order that this regiſter may produce its proper effect, a valve, or a *damper*, as it is commonly called, ſhould be placed in the chimney or canal by which the ſmoke is carried off; which damper ſhould be opened more or leſs, as the quantity of air is greater or leſs which is admitted into the fire-place. This regiſter and this damper will be found very uſeful in another reſpect, and that is, in putting out the Fire when there is no longer an occaſion for it; for, upon cloſing them both entirely, the Fire will be immediately extinguiſhed, and the half-conſumed Fuel, inſtead of being ſuffered to burn out to no purpoſe, will be ſaved.

Nearly the ſame effects as are produced by a damper may be produced without one, by cauſing the ſmoke, after it has quitted the fire-place, to *deſcend* ſeveral feet below the level of the grate on which the Fuel is burned, before it is permitted to go up the chimney.

There is another circumſtance of much importance which muſt be attended to in the conſtruction of fire-places; and that is, the proper diſpoſition of the Fuel; for in order that the combuſtion may go on well, it is neceſſary, not only that the Fuel be in its proper place, but alſo that it be properly diſpoſed;—that is to ſay, that the ſolid parts of the Fuel be of a juſt ſize, and that they be not placed too near each other, ſo as to prevent the free paſſage of the air between them, nor too far aſunder; and if the fire-place can be ſo contrived, that ſolid pieces of the inflamed Fuel, as they go on to be diminiſhed in ſize as they burn, may naturally fall together in the center of the fire-place without any aſſiſtance, it will be a great improvement, as I have found by experience. This may be done, in ſmall fire-places, (and in theſe it is more particularly neceſſary,) by burning the Fuel upon a grate in the form of a ſegment of a hollow ſphere, or of a diſh. (See the Figures 3 and 4. Plate I.) All thoſe I now uſe, except it be for fire-places which are very large, are of this form; and where wood is made uſe of for Fuel, it is cut into ſmall billets from 4 to 6 inches in length. Inſtead of a grate of iron, I have lately introduced grates, or rather hollow diſhes or pans, of earthen ware, perforated with a great number of holes for giving a paſſage to the air.

Theſe perforated earthen pans, which are made very thick and ſtrong, are incomparably cheaper than iron grates; and judging from the experience I have had of them, I am inclined to think they anſwer even better than the grates; indeed it appears to me not difficult to aſſign a reaſon why they ought to be better.

For large fire-places I have ſometimes uſed grates, the bars of which were common bricks placed edgewiſe, and theſe have been found to anſwer very well.

As only *that part of the air* which, entering the fire-place in a proper manner, and in a juſt quantity, and coming into actual contact with the burning Fuel, *is decompoſed*, contributes to the generation of heat; it is evident that all the air that finds its way into the fire-place, *and out of it again*, without being decompoſed, is a thief;—that it not only *contributes nothing* to the heat, but being itſelf heated at the expence of the Fire, and going off *hot* into the atmoſphere by the chimney, occaſions an actual loſs of heat; and this loſs is often very conſiderable, and the prevention of it is ſuch an object, that too much attention cannot be paid to it in the conſtruction of fire-places.

When the fire-place is cloſed on all ſides by a wall, and when the opening by which the Fuel is introduced is kept cloſed, no air can preſs in laterally upon the Fire; but yet, when the grate is larger than the heap of burning Fuel, which muſt often be the caſe, a great quantity of air may inſinuate itſelf by the ſides of the grate into the fire-place, without going through the Fire: But when, inſtead of an iron grate, a perforated hollow earthen pan is uſed, by making the bottom of the pan of a certain thickneſs, 2, 3, or 4 inches, for inſtance, and making all the air-holes point to one common center, (to the focus or center of the Fire,) this furtive entrance of cold air into the fire-place will, in a great meaſure, be prevented.

This evil may likewiſe be prevented when circular hollow iron grates are uſed, by narrowing the fire-place immediately under the grate, in the form of an inverted, truncated, hollow cone, the opening or diameter of which above being equal to the internal diameter of the circular rim of the grate, and that below (by which the air riſes to enter the fire-place) about *one-third* of that diameter. (See the Figure 5. Plate I.) This opening below, through which the air riſes, muſt be immediately under the center of the grate, and as near to it as poſſible; care muſt be taken, however, that a ſmall ſpace be left between the outſide or underſide of the iron bars which form the hollow grate, and the inſide ſurface of this inverted hollow cone, in order that the aſhes may ſlide down into the aſh-pit. Theſe directions are more peculiarly applicable to fire-places of a moderate ſize.

As to the form and ſize of the aſh-pit, theſe are matters of perfect indifference, provided, however, that it be large enough to give a free paſſage to the air neceſſary for feeding the Fire, and that the only paſſage into it, by which air can enter, is cloſed by a good door furniſhed with a regiſter. The neceſſity of being completely maſter of the paſſage, by which the air enters the fire-place, has already been ſufficiently explained.

It is perhaps unneceſſary for me to obſerve, that where perforated earthen pans are uſed inſtead of iron grates, the air-holes in the pans ought to be rather ſmaller above than below, in order that they may not be choaked up by the ſmall pieces of coal, and the aſhes which occaſionally fall through them into the aſh-pit.

One great advantage attending fire-places on the conſtruction here propoſed, is, that they ſerve equally well for every kind of Fuel. Wood, pit-coal, char-coal, turf, &c. may indifferently be uſed, and all of them with the ſame facility, and with the ſame advantages; or any two, or more, of theſe different kinds of Fuel, may be uſed at the ſame time without the ſmalleſt inconvenience;—or the Fire having been lighted with dry wood, or any other very inflammable material, the heat may afterwards be kept up by cheaper or more ordinary Fuel of a more difficult and ſlow combuſtion.—Some kinds of Fuel will perhaps be found moſt advantageous for making the pot boil, and others for keeping it boiling; and a very conſiderable ſaving will probably be found to reſult from paying due attention to this circumſtance. When the fire-place is ſo contrived as to ſerve equally well for all kinds of Fuel, this may be done without the leaſt difficulty or trouble.

I have juſt ſhown, that narrowing that part of the fire-place which lies below the grate, ſerves to make the air enter the fire in a more advantageous manner. This conſtruction has another advantage, perhaps ſtill more important; the heat which is projected downwards through the openings between the bars of the grate, inſtead of being permitted to eſcape into the aſh-pit, (where it would be loſt,) ſtriking againſt the ſides of this inverted hollow *cone*, it is there ſtopped, and afterwards riſes into the fire-place again with the current of air which feeds the Fire, or it is immediately reflected by this conical ſurface, and, after two or three bounds from ſide to ſide, is thrown up againſt the bottom of the boiler.

But in order to be able to form a clear and diſtinct idea upon this ſubject, it is neceſſary to examine with care all the circumſtances attending the generation of heat in the combuſtion of inflammable bodies, and to ſee in what manner, or under what form, the heat generated manifeſts itſelf, and how it may be collected, accumulated, confined, and directed.

This opens a wide field for philoſophical inquiry; but as theſe inveſtigations are not only curious and entertaining, but alſo uſeful and important in a high degree, I truſt my reader will pardon me for requeſting his particular attention while I endeavour to do juſtice to this moſt intereſting, but, at the ſame time, moſt abſtruſe and moſt difficult part of the ſubject I have undertaken to treat.

The heat generated in the combuſtion of Fuel manifeſts itſelf in two ways; namely, in the hot vapour which riſes from the Fire, with which it may be ſaid to be *combined*, and in the calorific rays which are thrown off from the Fire in all directions.—Theſe rays may, with greater propriety, be ſaid to be *calorific*, or *capable of generating heat*, in any body by which they are *ſtopped*, than to be called hot; for when they paſs freely through any medium, (as through a maſs of air, for inſtance,) they are not found to communicate any heat whatever to ſuch medium; neither do they appear to excite any conſiderable degree of heat in bodies from whoſe ſurfaces they are reflected; and in theſe reſpects they bear a manifeſt reſemblance to the rays emitted by the ſun.

What proportion this *radiant heat* (if I may be allowed to uſe ſo inaccurate an expreſſion) bears to that which goes off from burning bodies in the ſmoke and heated vapour, is not exactly known; it is certain, however, that the quantity of heat which goes off in the heated elaſtic fluids, viſible and inviſible, which riſe from a Fire, is much greater than that which all the calorific rays united would be capable of producing. But though the quantity of *radiant heat* is leſs than that exiſting as ſenſible Heat in the hot vapour, (and which, for the ſake of diſtinction, may be called *combined heat* *,) the former is ſtill much too conſiderable to be neglected.

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* It is evident that by the expreſſion here uſed "*combined Heat*," I do not mean what has been called *latent Heat*.

That the heat generated, or excited, by the calorific rays which proceed from burning bodies, is in fact conſiderable, is evident from the heat which is felt in a room warmed by a chimney Fire; for as all the heat, combined with the ſmoke and hot vapour, goes up the chimney, it is certain that the increaſe of heat in the room, occaſioned by the Fire, is entirely owing to the calorific rays thrown into it from the burning Fuel.

The activity of theſe rays may be ſhown in various ways, but in no way in a more ſtriking manner than by the following ſimple Experiment: When the Fire burns bright upon the hearth, let the arm be extended in a ſtraight line towards the center of the Fire, with the hand open, and all the fingers extended and pointing to the Fire. If the hand is not nearer the Fire than the diſtance of two or three yards, except the Fire be very large indeed, the heat will be ſcarcely perceptible; but if, without moving the arm, the wriſt be bent upwards ſo as to preſent the inſide or flat of the hand perpendicular to the Fire, the heat will not only be very ſenſibly felt, but, if the Fire be large, and if it burns clears and bright, it will be found to be ſo intenſe as to be quite inſupportable.

It is not, however, burning bodies alone that emit calorific rays. All bodies,—thoſe which are fixed and incombuſtible, as well as thoſe which are inflammable,—fluids as well as ſolids,—are found to throw off theſe rays in great abundance, as ſoon as they are heated to that degree which is neceſſary to their becoming luminous in the dark, or till they are red-hot.

Bodies even that are heated to a leſs degree than that which is neceſſary to their emitting *viſible* light, ſend off calorific rays in all directions. This is a matter of fact, that has been proved by experiment. Do all bodies, at all temperatures,—freezing mercury as well as melting iron,—continually emit theſe rays in greater or leſs quantities, or with greater or leſs velocities?—Are bodies cooled in conſequence of their emitting theſe rays?—Do theſe calorific rays *always* generate heat, even when the body, by which they are ſtopped or abſorbed, is hotter than that from which the rays proceeded?—But I forget that I promiſed not to involve myſelf in abſtruſe ſpeculation.—To return then:—Whatever may be the nature of the rays emitted by burning Fuel, as *one* of their *known properties* is to generate heat, they ought certainly to be very particularly attended to in every arrangement in which the Economy of Heat, or of Fuel, is a principal object in view.

As theſe calorific rays generate heat in the body by which they are *ſtopped or abſorbed*, and not in the medium through which they paſs, it is neceſſary to diſpoſe thoſe bodies which are deſigned for ſtopping them, in ſuch a manner that they may eaſily and *neceſſarily* communicate the heat they thus acquire to the body upon which it is intended that it ſhould operate.

The cloſed fire-places which I have recommended, and which will hereafter be more particularly deſcribed, will anſwer this purpoſe completely. The Fire being cloſed in theſe fire-places on every ſide, as well below the grate as laterally, and in ſhort every where, except where the bottom of the boiler preſents itſelf to the Fire, none of theſe rays can poſſibly eſcape; and as the materials of which the fire-place is conſtructed, (bricks and mortar,) are bad conductors of heat, but a ſmall part of the heat generated in the combuſtion of the Fuel will be abſorbed and tranſmitted by them into the interior parts of the wall, there to be diſperſed and loſt. But the confining of heat is a matter of ſufficient importance to deſerve being treated in a ſeparate Chapter.

CHAP. III.

Of the Means of CONFINING HEAT, *and* DIRECTING ITS OPERATIONS.—*Of Conductors and Non-conductors of Heat.—Common Atmoſpheric Air a good Non-conductor of Heat, and may be employed with great Advantage for confining it—is employed by Nature for that Purpoſe, in many Inſtances—is the principal Cauſe of the Warmth of Natural and Artificial Clothing—is the ſole Cauſe of the Warmth of Double Windows.—Great Utility of Double Windows and Double Walls—they are equally uſeful in Hot Countries as in Cold.*—ALL ELASTIC FLUIDS *Non-conductors of Heat.*—STEAM *proved by Experiment to be a Non-conductor of Heat.*—FLAME *is alſo a Non-conductor of Heat.*

THAT HEAT paſſes more freely through ſome bodies than through others, is a fact well known; but the cauſe of this difference in the conducting powers of bodies with reſpect to Heat, has not yet been diſcovered.

The utility of giving a wooden handle to a teapot or coffee-pot of metal, or of covering its metallic handle with leather, or with wood, is well known: But the difference in the conducting powers of various bodies with regard to Heat, may be ſhewn by a great number of very ſimple experiments;—ſuch as are in the power of every one to make at all times and in all places, and almoſt without either trouble or expence.

If an iron nail, and a pin of wood, of the ſame form and dimenſions, be held ſucceſſively in the flame of a candle, the difference in the conducting powers of the metal and of wood will manifeſt itſelf in a manner in which there will be no room left for doubt. As ſoon as the end of the nail, which is expoſed in the flame of the candle, begins to be heated, the other end of it will grow ſo hot as to render it impoſſible to hold it in the hand without being burnt; but the wood may be held any length of time in the ſame ſituation without the leaſt inconvenience; and, even after it has taken fire, it may be held till it is almoſt entirely conſumed; for the uninflamed wood will not grow hot, and, till the flame actually comes in contact with the fingers, they will not be burnt. If a ſmall ſlip or tube of glaſs be held in the flame of the candle in the ſame manner, the end of the glaſs by which it is held will be found to be more heated than the wood, but incomparably leſs ſo than the pin or nail of metal;—and among all the various bodies that can be tried in this manner, no two of them will be found to give a paſſage to Heat through their ſubſtances with exactly the ſame degree of facility *.

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* To ſhow the relative conducting power of the different metals, Doctor Ingenhouz contrived a very pretty experiment. He took equal

To confine Heat is nothing more than to prevent its eſcape out of the hot body in which it exiſts, and in which it is required to be retained; and this can only be done by ſurrounding the hot body by ſome covering compoſed of a ſubſtance through which Heat cannot paſs, or through which it paſſes with great difficulty. If a covering could be found perfectly impervious to Heat, there is reaſon to believe that a hot body, completely ſurrounded by it, would remain hot for ever; but we are acquainted with no ſuch ſubſtance; nor is it probable that any ſuch exiſts.

Thoſe bodies in which Heat paſſes freely or rapidly, are called *Conductors* of Heat; thoſe in which it makes its way with great difficulty, or very ſlowly, *Non-conductors*, or bad Conductors of Heat. The epithets, good, bad, indifferent, excellent, &c. are applied indifferently to *conductors*, and to *non-conductors*. A good conductor, for inſtance, is one in which Heat paſſes very freely; a good non-conductor is one in which it paſſes with great difficulty; and an indifferent conductor may likewiſe be called, without any impropriety, an indifferent non-conductor.

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equal cylinders of the different metals, (being ſtrait pieces of ſtout wire, drawn through the ſame hole, and of the ſame length,) and dipping them into melted wax, covered them with a thin coating of the wax. He then held one end of each of theſe cylinders in boiling water, and obſerved how far the coating of wax was melted by the Heat communicated through the metal, and with what celerity the Heat paſſed.

Thoſe bodies which are the worſt conductors, or rather the beſt non-conductors of Heat, are beſt adapted for forming coverings for confining Heat.

All the metals are remarkably good conductors of Heat;—wood, and in general all light, dry, and ſpungy bodies, are non-conductors: Glaſs, though a very hard and compact body, is a non-conductor. Mercury, water, and liquids of all kinds, are conductors; but air, and in general all elaſtic fluids, *ſteam* even not excepted, are non-conductors.

Some experiments which I have lately made, and which have not yet been publiſhed, have induced me to ſuſpect that *water*, mercury, and all other non-elaſtic fluids, do not permit Heat to paſs through them from particle to particle, as it undoubtedly paſſes through ſolid bodies, but that their apparent conducting powers depend eſſentially upon the extreme mobility of their parts; in ſhort, that they rather *tranſport* Heat than allow it a paſſage. But I will not anticipate a ſubject which I propoſe to treat more fully at ſome future period.

The conducting power of any ſolid body in one ſolid maſs, is much greater than that of the ſame body reduced to a powder, or divided into many ſmaller pieces: An iron bar, or an iron plate, for inſtance, is a much better conductor of Heat than iron filings; and ſaw-duſt is a better non-conductor than wood. Dry wood-aſhes is a better non-conductor than either; and very dry charcoal reduced to a fine powder is one of the beſt non-conductors known; and as charcoal is perfectly incombuſtible when confined in a ſpace where freſh air can have no acceſs, it is admirably well calculated for forming a barrier for confining Heat, where the Heat to be confined is intenſe.

But among all the various ſubſtances of which coverings may be formed for confining Heat, none can be employed with greater advantage than common atmoſpheric air. It is what Nature employs for that purpoſe; and we cannot do better than to imitate her.

The warmth of the wool and fur of beaſts, and of the feathers of birds, is undoubtedly owing to the air in their interſtices; which air, being ſtrongly attracted by theſe ſubſtances, is confined, and forms a barrier which not only prevents the cold winds from approaching the body of the animal, but which oppoſes an almoſt inſurmountable obſtacle to the eſcape of the Heat of the animal into the atmoſphere. And in the ſame manner the air in ſnow ſerves to preſerve the Heat of the earth in winter. The warmth of all kinds of artificial clothing may be ſhown to depend on the ſame cauſe; and were this circumſtance more generally known, and more attended to, very important improvements in the Management of Heat could not fail to reſult from it. A great part of our lives is ſpent in guarding ourſelves againſt the extremes of heat and of cold, and in operations in which the uſe of Fire is indiſpenſable; and yet how little progreſs has been made in that moſt uſeful and moſt important of the arts,—the Management of Heat!

Double windows have been in uſe many years in moſt of the northern parts of Europe, and their great utility, in rendering the houſes furniſhed with them warm and comfortable in winter, is univerſally acknowledged,—but I have never heard that any body has thought of employing them in hot countries to keep their apartments cool in ſummer;—yet how eaſy and natural is this application of ſo ſimple and ſo uſeful an invention!—If a double window can prevent the heat which is *in* a room from paſſing *out of it*, one would imagine it could require no great effort of genius to diſcover that it would be equally efficacious for preventing the Heat *without* from coming *in*. But natural as this concluſion may appear, I believe it has never yet occurred to any body; at leaſt I am quite certain that I have never ſeen a double window either in Italy, or in any other hot country I have had occaſion to viſit *.

But the utility of double windows and double walls, in hot as well as in cold countries, is a matter of ſo much importance that I ſhall take occaſion to treat it more fully in another place. In the mean time, I ſhall only obſerve here, that it is the *confined air* ſhut up between the two windows, and not the double glaſs plates, that renders the paſſage of Heat through them ſo difficult. Were it owing to the increaſed thickneſs of the glaſs, a ſingle pane of glaſs twice as thick would anſwer the ſame purpoſe; but the increaſed thickneſs of the glaſs of which a window is formed, is not found to have any ſenſible effect in rendering a room warmer.

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* When double windows are uſed in hot countries, to keep dwelling-houſes cool, great care muſt be taken to ſcreen thoſe windows from the ſun's direct rays, and even from the ſtrong light of day, otherwiſe they will produce effects directly contrary to thoſe intended. This may eaſily be done, either by Venetian blinds or by awnings. In all caſes where rooms are to be kept cool in hot weather, the leſs light that is permitted to enter them, the cooler they will be.

But air is not only a non-conductor of Heat, but its non-conducting power may be greatly increaſed. To be able to form a juſt idea of the manner in which air may be rendered a worſe conductor of Heat, or, which is the ſame thing, a better non-conductor of it than it is in its natural unconfined ſtate, it will be neceſſary to conſider *the manner* in which Heat paſſes through air. Now it appears, from the reſult of a number of experiments which I made with a view to the inveſtigation of this ſubject, and which are publiſhed in a Paper read before the Royal Society *, that though the particles of air, *each particle for itſelf*, can receive Heat from *other bodies*, or communicate it to them, yet there is no communication of Heat *between one particle of air and another particle of air*. And from hence it follows, that though air may, and certainly does, *carry off* Heat, and *tranſport it* from one place, or from one body to another, yet a maſs of air in a quieſcent ſtate, or with all its particles at reſt, *could it remain in that ſtate*,—would be totally impervious to Heat; or ſuch a maſs of air would be a perfect non-conductor.

Now if heat paſſes in a maſs of air merely in conſequence of the motion it occaſions in that air,—if it be *tranſported,—not ſuffered to paſs,*—in that caſe, it is clear, that whatever can obſtruct and impede the internal motion of the air, muſt tend to diminiſh its conducting power: And this I have found to be the caſe in fact. I found that a certain quantity of Heat which was able to make its way through a wall, or rather a ſheet of confined air $\frac{1}{2}$ an inch thick in $9\frac{3}{5}$ minutes, required $21\frac{2}{5}$ minutes to make its way through the ſame wall, when the internal motion of this air was impeded by mixing with it $\frac{1}{56}$ part of its bulk of eider-down,—of very fine fur, or of fine ſilk, as ſpun by the worm.

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* See the Philoſophical Tranſactions, 1792.

But in mixing bodies with air, in order to impede its internal motion, and render it more fit for confining Heat, ſuch bodies only muſt be choſen as are themſelves non-conductors of Heat, otherwiſe they will do more harm than good, as I have found by experience. When, inſtead of making uſe of eider-down, fur, or fine ſilk, for impeding the internal motion of the confined air, I uſed an equal volume of exceedingly fine ſilver-wire flatted, (being the ravellings of gold or ſilver lace,) the paſſage of the Heat through the barrier, ſo far from being impeded, was remarkably facilitated by this addition; the Heat paſſing through this compound of air and fine threads of metal much ſooner than it would have made its way through the air alone.

Another circumſtance to be attended to in the choice of a ſubſtance to be mixed with air, in order to form a covering or barrier for confining Heat, is the fineneſs or ſubtility of its parts; for the finer they are, the greater will be their ſurface in proportion to their ſolidity, and the more will they impede the motions of the particles of the air. Coarſe horſe-hair would be found to anſwer much worſe for this purpoſe than the fine fur of a beaver, though it is not probable that there is any eſſential difference in the chymical properties of thoſe two kinds of hair.

But it is not only the fineneſs of the parts of a ſubſtance, and its being a non-conductor, which render it proper to be employed in the formation of covering to confine Heat;—there is ſtill another property, more occult, which ſeems to have great influence in rendering ſome ſubſtances better fitted for this uſe than others; and this is a certain attraction which ſubſiſts between certain bodies and air. The obſtinacy with which air adheres to the fine fur of beaſts and to the feathers of birds, is well known; and it may eaſily be proved that this attraction muſt aſſiſt very powerfully in preventing the motion of the air concealed in the interſtices of thoſe ſubſtances, and conſequently in impeding the paſſage of Heat through them.

Perhaps there may be another ſtill more hidden cauſe which renders one ſubſtance better than another for confining Heat. I have ſhown by a direct and unexceptionable Experiment, that Heat can paſs through the Torricellian vacuum *, though with rather more difficulty than in air (the con-

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* See my Experiments on Heat, publiſhed in the Philoſophical Tranſactions, vol. lxxvi.

ducting power of air being to that of a Torricellian vacuum as 1000 to 604, or as 10 to 6, very nearly); but if Heat can paſs where there is no air, it muſt in that caſe paſs by a medium more ſubtile than air; a medium which moſt probably pervades all ſolid bodies with the greateſt facility, and which muſt certainly pervade either the glaſs or the mercury employed in making a Torricellian vacuum.

Now, if there exiſts a medium more ſubtile than air, by which Heat may be conducted, is it not poſſible that there may exiſt a certain affinity between that medium and ſenſible bodies? a certain attraction or coheſion by means of which bodies in general, or ſome kinds of bodies in particular, may, ſome how or other, impede this medium in its operations in conducting or tranſporting Heat from one place to another? It appeared from the reſult of ſeveral of my Experiments, of which I have given an account in detail in my paper before mentioned, publiſhed in the year 1786 in the LXXVIth Vol. of the Philoſophical Tranſactions, that the conducting power of a Torricellian vacuum is to that of air as 604 to 1000:—but I found by a ſubſequent Experiment, (ſee my ſecond Paper on Heat, publiſhed in the Philoſophical Tranſactions for the year 1792,)—that 55 parts, in bulk of air, with 1 part of fine raw ſilk, formed a covering for confining Heat, the conducting power of which was to that of air as 576 to 1284; or as 448 to 1000. Now, from the reſult of this laſt-mentioned Experiment, it ſhould ſeem that the introduction into the ſpace through which the Heat paſſed, of ſo ſmall a quantity of raw ſilk as $\frac{1}{56}$ part of the volume, or capacity of that ſpace, rendered that ſpace (which now contained 55 parts of air and 1 part of ſilk) more impervious to Heat than even a Torricellian vacuum.—The ſilk muſt therefore not only have completely deſtroyed the conducting power of the air, but muſt alſo at the ſame time have very ſenſibly impaired that of the etherial fluid which probably occupies the interſtices of air, and which ſerves to conduct Heat through a Torricellian vacuum: For a Torricellian vacuum was a better conductor of Heat, than this medium, in the proportion of 604 to 448. But I forbear to enlarge upon this ſubject, being ſenſible of the danger of reaſoning upon the properties of a fluid whoſe exiſtence even is doubtful; and feeling that our knowledge of the nature of Heat, and of the manner in which it is communicated from one body to another, is much too imperfect and obſcure to enable us to purſue theſe ſpeculations with any proſpect of ſucceſs or advantage.

Whatever may be the *manner* in which Heat is communicated from one body to another, I think it has been ſufficiently proved that it paſſes with great difficulty through confined air; and the knowledge of this fact is very important, as it enables us to take our meaſures with certainty and with facility for confining Heat, and directing its operations to uſeful purpoſes.

But atmoſpheric air is not the only non-conductor of Heat. All kinds of air, artificial as well as natural, and in general all elaſtic fluids, *ſteam not excepted*, ſeem to poſſeſs this property in as high a degree of perfection as atmoſpheric air.

That ſteam is not a conductor of Heat, I proved by the following Experiment: A large globular bottle being provided, of very thin and very tranſparent glaſs, with a narrow neck, and its bottom drawn inward ſo as to form a hollow hemiſphere about 6 inches in diameter; this bottle, which was about 8 inches in diameter externally, being filled with cold water, was placed in a ſhallow diſh, or rather plate, about 10 inches in diameter, with a flat bottom formed of very thin ſheet braſs, and raiſed upon a tripod, and which contained a ſmall quantity (about $\frac{2}{10}$ of an inch in depth) of water; a ſpirit lamp being then placed under the middle of this plate, in a very few minutes the water in the plate began to boil, and the hollow formed by the bottom of the bottle was filled with clouds of ſteam, which, after circulating in it with ſurpriſing rapidity 4 or 5 minutes, and after forcing out a good deal of air from under the bottle, began gradually to clear up. At the end of 8 or 10 minutes (when, as I ſuppoſed, the air remaining with the ſteam in the hollow cavity formed by the bottom of the bottle, had acquired nearly the ſame temperature as that of the ſteam) theſe clouds totally diſappeared; and, though the water continued to boil with the utmoſt violence, the contents of this hollow

low cavity became ſo perfectly inviſible, and ſo little appearance was there of ſteam, that, had it not been for the ſtreams of water which were continually running down its ſides, I ſhould almoſt have been tempted to doubt whether any ſteam was actually generated.

Upon lifting up for an inſtant one ſide of the bottle, and letting in a ſmall quantity of cold air, the clouds inſtantly returned, and continued circulating ſeveral minutes with great rapidity, and then gradually diſappeared as before. This Experiment was repeated ſeveral times, and always with the ſame reſult; the ſteam always becoming viſible when cold air was mixed with it, and afterwards recovering its tranſparency when, part of this air being expelled, that which remained had acquired the temperature of the ſteam.

Finding that cold air introduced under the bottle cauſed the ſteam to be partially condenſed, and clouds to be formed, I was deſirous of ſeeing what viſible effects would be produced by introducing a cold ſolid body under the bottle. I imagined that if ſteam was a conductor of Heat, ſome part of the Heat in the ſteam paſſing out of it into the cold body, clouds would of courſe be formed; but I thought if ſteam was a *non-conductor* of Heat,—that is to ſay, *if one particle of ſteam could not communicate any part of its Heat to its neighbouring particles*, in that caſe, as the cold body could only affect the particles of ſteam *actually in contact with it*, no cloud would appear; and the reſult of the

Experiment ſhewed that ſteam is in fact a *non-conductor of Heat;* for, notwithſtanding the cold body uſed in this Experiment was very large and very cold, being a ſolid lump of ice nearly as large as an hen's egg, placed in the middle of the hollow cavity under the bottle, upon a ſmall tripod or ſtand made of iron wire; yet as ſoon as the clouds which were formed in conſequence of the unavoidable introduction of cold air in lifting up the bottle to introduce the ice, were diſſipated, which ſoon happened, the ſteam became ſo perfectly tranſparent and inviſible, that *not the ſmalleſt appearance of cloudineſs was to be ſeen any where,* not even about the ice, which, as it went on to melt, appeared as clear and as tranſparent as a piece of the fineſt rock cryſtal.

This Experiment, which I firſt made at Florence, in the month of November 1793, was repeated ſeveral times in the preſence of Lord Palmerſton, who was then at Florence, and Monſ. de Fontana *.

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* The bottle made uſe of in this Experiment, though it appeared very large externally, contained but a very ſmall quantity of water, owing to its bottom being very much drawn inwards. As the hollow cavity under the bottle of the bottle (which, as I juſt obſerved, was nearly in the form of a hemiſphere, and 6 inches in diameter) ſerved as a receiver for confining the ſteam which roſe from the boiling water in the plate, it may perhaps be imagined that a common glaſs receiver in the form of a bell, ſuch as are uſed in Pneumatical Experiments, might anſwer as well as this bottle; I thought ſo myſelf, but upon making the experiment I found my miſtake. A common receiver will anſwer perfectly well for confining the ſteam, but the glaſs ſoon becomes ſo hot that the drops of water which are formed upon its internal ſurface, in conſequence of the condenſation of the ſteam, inſtead of running down the ſides of the receiver in clear tranſ-

In theſe Experiments the air was not entirely expelled from under the bottle; on the contrary, a conſiderable quantity of it remained mixed with the ſteam even after the clouds had totally diſappeared, as I found by a particular Experiment made with a view to aſcertain that fact; but that circumſtance does not render the reſult of this Experiment leſs curious, on the contrary I think it tends to make it more ſurpriſing. It ſhould ſeem that neither the maſs of ſteam, nor that of air, were at all cooled by the body of ice which they ſurrounded, for if the air had been cooled, (in maſs,) it ſeems highly probable that the clouds would have returned.

The reſults of theſe Experiments compared with thoſe formerly alluded to, in which I had endeavoured to aſcertain the moſt advantageous forms for boilers, opened to me an entirely new field for ſpeculation and for improvement in the Management of Fire. They ſhewed me that not only cold air, but alſo hot air, and hot ſteam, and hot mixtures of air and ſteam, are non-conductors of Heat; conſequently, that the hot vapour which

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tranſparent ſtreams, form blotches and ſtreaks, which render the glaſs ſo opaque that nothing can be ſeen diſtinctly through it; and this of courſe completely fruſtrates the main deſign of the Experiment; but the cold water in the bottle keeping the glaſs cool, the condenſation of the ſteam upon the ſides of the hollow cavity formed by the bottom of the bottle, goes on more regularly, and the ſtreams of water which are continually running down the ſides of the glaſs, uniting together, form one tranſparent ſheet of water, by which means every thing that goes on under the bottle may be diſtinctly ſeen.

riſes from burning fuel, and even the *flame itſelf, is a non-conductor of Heat.*

This may be thought a bold aſſertion, but a little calm reflection, and a careful examination of the phenomena which attend the combuſtion of Fuel, and the communication of Heat by flame, will ſhow it to be well-founded; and the advantages that may be derived from the knowledge of this fact are of very great importance indeed. But this ſubject deſerves to be thoroughly inveſtigated.

CHAP. IV.

Of the MANNER *in which* HEAT *is* COMMUNICATED *by* FLAME *to other Bodies.—Flame acts on Bodies in the ſame manner as a hot Wind.—The Effect of a Blow-pipe in increaſing the Activity of Flame explained, and illuſtrated by Experiments.—A Knowledge of the Manner in which Heat is communicated by Flame neceſſary in order to determine the moſt advantageous Forms for Boilers.—General Principles on which Boilers of all Dimenſions ought to be conſtructed.*

IF FLAME be merely vapour, or a mixture of air and ſteam heated red-hot, as *air* and *ſteam* are both non-conductors of heat, there ſeems to be no difficulty in conceiving that *Flame* may, notwithſtanding its great degree of heat, ſtill retain the properties of its component fluids, and remain *a non-conductor of Heat.* The non-conducting power of air does not appear to be at all impaired by being heated to the temperature of boiling water; and I ſee no reaſon why that property in air, or in any other elaſtic fluid, ſhould be impaired by any augmentation of temperature however great. If ſteam, or if air, at the temperature of 212 degrees of Fahrenheit's thermometer, be a non-conductor of heat, why ſhould it not remain a non-conductor at that of 1000 degrees, or when heated red-hot? I confeſs I do not ſee how a body *could* be deprived of a property ſo eſſential, without being at the ſame time totally changed; and I believe nobody will imagine that either air or ſteam undergo any chymical change merely by being heated to the temperature of red-hot iron. But without inſiſting upon theſe reaſonings, however concluſive I may think them, I ſhall endeavour to ſhow, from experiment and obſervation, in ſhort to *prove*, that Flame is in fact a non-conductor of Heat.

Taking it for granted,—what I imagine will not be denied,—that air is a non-conductor of heat, at leaſt in the ſenſe I have uſed that appellation, I ſhall endeavour to ſhow that Flame acts preciſely in the ſame manner as a hot wind would do in communicating heat, and in no other way; and if I ſucceed in this, I fancy I may conſider the propoſition as ſufficiently proved.

The effect of a blaſt of cold air in cooling any hot body expoſed to it is well known, and the cauſes of this effect may eaſily be traced to that property of air which renders it a non-conductor of heat; for if the particles of cold air in contact with a hot body, could, with perfect facility, give the heat they acquire from the hot body to other particles of air by which they are immediately ſurrounded, and theſe again to others, and ſo on, the heat would be carried off *as faſt as the hot body could part with it*, and any motion of the particles of the air,—any wind, or blaſt, would not ſenſibly facilitate or haſten the cooling of the body; and by a parity of reaſoning it may be ſhown, that if Flame were in fact a perfect conductor of heat, any cold body plunged into it would always be heated *as faſt as that body could receive heat*; and neither any motion of the internal parts of the Flame, nor the velocity with which it impinged againſt the cold body, could have any ſenſible effect either to facilitate or accelerate the heating of the body. But if Flame be a non-conductor of heat, its action will be exactly ſimilar to that of a hot wind, and conſequently much will depend upon the manner in which it is applied to any body intended to be heated by it.—Thoſe particles of it *only* which are in *actual contact* with the body will communicate heat to it; and the greater the number of different particles of the Flame that are brought into contact with it, the greater will be the quantity of heat communicated: Hence the importance of cauſing the Flame to impinge with force againſt the body to be heated, and to ſtrike it in ſuch a manner that its current may be broken, and that whirlpools may be formed in it; for the rapid motion of the Flame cauſes a quick ſucceſſion of hot particles; and admitting our aſſumed principles to be true, it is quite evident that every kind of internal motion among the particles of the Flame by which it can be agitated, muſt tend very powerfully to accelerate the communication of the heat.

The effect of a blow-pipe is well known, but I do not think that the *manner* in which it increaſes the *action* of *Flame* has ever been ſatisfactorily explained. It has generally been imagined, I believe, that the current of freſh air which is forced through the Flame by a blow-pipe actually increaſes the quantity of heat; I rather ſuppoſe it does little more than direct the heat *actually exiſting in the Flame* to a given point. A current of air cannot *generate* heat, without at the ſame time being decompoſed; and in order to its being decompoſed in a fire, it muſt be brought into actual contact with the burning Fuel, or at leaſt with the uninflamed inflammable vapour which riſes from it:—But can it be ſuppoſed that there can be any thing inflammable, and not actually inflamed, in the clear, bright, and perfectly tranſparent Flame of a wax-candle?—A blow-pipe has however as ſenſible an effect, when directed againſt the clear Flame of a wax-candle, as when it is employed to encreaſe the action of a common glaſs-worker's lamp.

Conceiving that the diſcovery of the *manner* in which the current of air from a blow-pipe ſerves to increaſe the intenſity of the action of the Flame could not fail to throw much light upon the ſubject under conſideration,—namely, the inveſtigation of *the manner* in which heat is communicated to bodies by Flame,—I made the following Experiments, the reſults of which I conceive to be deciſive.

Concluding that the current of air from a blow-pipe, directed againſt the Flame of any burning body, could tend to increaſe the intenſity of the action of the Flame only in one or both of theſe two ways, namely, by increaſing its *action* upon the body againſt which it is directed; or by actually increaſing the *quantity* of heat generated in the combuſtion of the Fuel; a method occurred to me by which I thought it poſſible to determine, by actual experiment, to which of theſe cauſes the effect in queſtion is owing, or how much each of them might contribute to it. To do this I filled a large bladder, containing above a gallon, with *fixed air*, which, as is well known, is totally unfit for ſupporting the combuſtion of inflammable bodies, and which, of courſe, could not be ſuſpected of *adding* any heat to a Flame againſt which a current of it ſhould be directed; I imagined therefore that if a blow-pipe ſupplied with this air, on being directed againſt the Flame of a candle, ſhould be found to produce nearly the ſame effect as when common air is uſed for the ſame purpoſe, it would prove to a demonſtration that the augmentation of the intenſity of the action, or activity of the Flame which ariſes from the uſe of a blow-pipe, is owing to the agitation of the Flame,—to its being directed to a point,—to the impetuoſity with which it is made to ſtrike againſt the body that is heated by it,—and to the rapid ſucceſſion of freſh particles of this hot vapour, and not to any *poſitive increaſe of heat*.

A blow-

A plow-pipe being attached to the bladder containing fixed air, the end of this pipe was directed to the clear brilliant flame of a wax candle, which had juſt been ſnuffed; and, by compreſſing the bladder, the Flame was projected againſt a ſmall tube of glaſs, which was very ſoon made red-hot, and even melted.

Having repeated this Experiment ſeveral times, and having found how long it required to melt the tube when the Flame of the candle was forced againſt it by a blaſt of *fixed air*, I now varied the experiment, by making uſe of common atmoſpheric air, inſtead of fixed air; taking care to employ the ſame candle and the ſame blow-pipe uſed in the former experiments, and even making uſe of the bladder, in order that the experiments being exactly ſimilar, and differing only in the kinds of air made uſe of, the effect of that difference might be diſcovered and eſtimated.

The reſults of theſe experiments were very intereſting; and proved in a deciſive manner, that the effect of a blow-pipe, *when applied to clear Flame*, ariſes not from any real augmentation of heat, but merely from the encreaſed activity of the Flame, in conſequence of its being impelled with force, and broken in eddies on the ſurface of the body againſt which it is made to act; the effect of the blow-pipe on theſe experiments being to all appearance quite as great when fixed air was made uſe of, (which *could not* increaſe the quantity of heat,) as when atmoſpheric air was uſed.

But conceiving the determination of this queſtion relative to the manner which Flame communicates heat, to be a matter of much importance, I did not reſt my inquiries here: I repeated the experiments very often, and varied them in a great number of different ways; ſometimes making uſe of fixed air; ſometimes of atmoſpheric air; and at other times uſing dephlogiſticated air; and common air rendered unfit for the ſupport of animal life and of combuſtion, by burning a candle in it till the candle went out.

It would take up too much time to give an account in detail of all theſe experiments; I ſhall therefore content myſelf with merely obſerving that they all tended to ſhow that the effect of a blow-pipe *uſed in the manner here deſcribed*, is owing to the direction and velocity it gives to the Flame againſt which it is employed, and not to any real increaſe of heat.

It muſt be remembered that the principal object I had in view in theſe experiments was to diſcover the *manner* in which Flame communicates heat to other bodies, and by what means that communication may be facilitated.—Were it required to increaſe the intenſity of the heat by *blowing the fire*, the current of air muſt be applied in ſuch a manner as to expedite the combuſtion; it muſt be directed to the inflamed ſurface of the burning Fuel, and not to the red-hot vapour or flame which riſes from it, and in which the combuſtion is moſt probably already quite complete; and in this caſe there is no doubt but the effect produced by blowing would depend much upon the quality of the air made uſe of.

The reſults of the foregoing experiments with the blow-pipe will, I am confident, be thought quite concluſive by thoſe who take the trouble to conſider them attentively,—and the advantages that may be derived from the knowledge of the fact eſtabliſhed by them are very obvious. If Flame, or the hot vapour which ariſes from burning bodies, be a non-conductor of heat;—and if, in order to communicate its heat to any other body, it be neceſſary that its particles *individually* be brought into actual contact with that body; it is evident that the form of a boiler, and of its fire-place, muſt be matters of much importance; and that *that form* muſt be moſt advantageous, which is beſt calculated to produce an internal motion in the Flame, and to bring alternately as many of its particles as poſſible into contact with the body which is to be heated by it. The boiler muſt not only have as large a ſurface as poſſible, but it muſt be of ſuch a form as to cauſe the Flame which embraces it—to impinge againſt it with force—to break againſt it—and to play over its ſurface in eddies and whirlpools.

It is therefore againſt the *bottom* of a boiler, and not againſt its ſides, that the principal efforts of the Flame muſt be directed; for when the Flame, or hot vapour, is permitted to riſe freely by the vertical ſides of a boiler, it ſlides over its ſurface very rapidly, and there being no obſtacle in the way to break the Flame into eddies and whirlpools, it glides quietly on like a ſtream of water in a ſmooth canal; and the ſame hot particles of this vapour which happen to be in immediate contact with the ſides of the boiler at its bottom or lower extremity, being continually preſſed againſt the ſurface of the boiler as they are forced upwards by the riſing current, prevent other hot particles from approaching the boiler; ſo that by far the greateſt part of the heat in the Flame and hot vapour which riſe from the Fire, inſtead of entering the boiler, goes off into the atmoſphere by the chimney, and is totally loſt.

The amount of this loſs of heat, ariſing from the faulty conſtruction of boilers and their fire-places, may be eſtimated from the reſults of the Experiments recorded in the following Chapter.

CHAP. V.

An Account of Experiments made with Boilers and Fire-places of various Forms and Dimenſions; together with Remarks and Obſervations on their Reſults, and on the Improvements that may be derived from them.—An Account of ſome Experiments made on a very large Scale in a Brew-houſe Boiler.—An Account of a Brew-houſe Boiler conſtructed and fitted up on an improved Plan.—Reſults of ſeveral Experiments that were made with this new Boiler.—Of the Advantage in regard to the Economy of Fuel in boiling Liquids, which ariſes from performing that Proceſs on a large Scale.—Theſe Advantages are limited.—An Account of an Alteration that was made in the new Brew-houſe Boiler, with a view to the SAVING OF TIME *in cauſing its Contents to boil.—Experiments ſhowing the Effects produced by theſe Alterations.—An Eſtimate of the* RELATIVE QUANTITIES OF HEAT *producible from* COAKS.—PIT-COAL—CHARCOAL, *and* OAK.—*A Method of eſtimating the Quantity of Pit-coal which would be neceſſary to perform any of the Proceſſes mentioned in this Eſſay, in which Wood was uſed as Fuel.—An Eſtimate of the* TOTAL QUANTITIES *of Heat producible in the Combuſtion of different Kinds of Fuel; and of the real Quantities of Heat*

Heat which are loſt, under various Circumſtances, in culinary Proceſſes.

WHAT has been ſaid in the foregoing Chapter will, I truſt, be ſufficient to give my reader a clear and diſtinct idea of the ſubject under conſideration, in all its various details and connections, and enable him to comprehend, without the ſmalleſt difficulty, every thing I have to add on this ſubject; and particularly to diſcover the different objects I had in view in the Experiments of which I am now about to give an account, and to judge with facility and certainty of the concluſions I have drawn from their reſults.

Theſe Experiments, though they occupy ſo many pages in this Eſſay, are but a ſmall part of thoſe I have made, and cauſed to be made under my direction, on the ſubject of Heat, during the laſt ſeven years. Were I to publiſh them all, with all their details, as they are recorded in the regiſter that has been kept of them, they would fill ſeveral volumes.

It was moſt fortunate for me that this regiſter is very voluminous; for had it not been ſo, I ſhould in all probability have taken it with me to England laſt year, and in that caſe I ſhould have loſt it, with the reſt of my papers, in the trunk of which I was robbed in paſſing through St. Paul's Church-yard, on my arrival in London after an abſence of eleven years*.

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* I have many reaſons to think that theſe papers are ſtill in being;—what an everlaſting obligation ſhould I be under to the perſon who would cauſe them to be returned to me!

As I foreſaw, when I firſt began my inquiries reſpecting Heat, that I ſhould have occaſion to make many Experiments on boiling Liquids, to facilitate the regiſtering of them I formed a Table, (which I had printed,) in which, under various heads, every circumſtance relative to any common Experiment of the kind in queſtion could be entered with much regularity, and with little trouble.

As this Table may be uſeful to others who may be engaged in ſimilar purſuits, and as the publiſhing of it will alſo tend to give my reader a more perfect idea of the manner in which my Experiments were conducted, I ſhall (as an example) give an account of one Experiment, *in the ſame form* in which it was regiſtered in one of theſe printed Tables.

Theſe Tables, as they are printed for uſe, (on detached ſheets,) occupy one ſide of half a ſheet of common folio writing-paper.

An EXPERIMENT on the MANAGEMENT of FIRE in BOILING LIQUIDS; Made at [Munich], in [a Brew-houſe belonging to the Elector], the [15th] of [April 1795].

Time of the Day.		Fuel put into the Fire-place.		Temperature of the Liquid.	Contents of the Boiler.		
Hour.	Minutes.	Numb. of Pieces.	In Weight.		Kind of Liquid, &c.	Meaſure.	In Weight.
			lbs.				lbs.
9	15	29	100	60°	[Water].	6984	12508
—	30	6	50	70°			
—	41	6	50	—			
10	5	7	50	92°			
—	25	7	50	105°			
—	46	7	50	120°			
11	0	7	50	130°			
—	15	7	50	—			
—	26	8	50	145°			
—	43	7	50	155°			
—	50	7	50	163°			
12	5	8	50	173°			
—	17	7	50	183°			
—	31	15	100	192°			
—	55	—	—	[boiled].			
1	38	7	50	—			
2	35	7	50	—			
3	38	—	—	[ceaſed boiling].			

Height of the Barometer, 26 $\frac{5}{12}$ Inches; of Thermometer, 58°.

DIMENSIONS OF THE BOILER.

Diameter { above——— or long——— 10 Feet.
below——— and wide——— 8 Feet.

Deep, 4 Feet.—Was conſtructed of [Copper], and weighed [not known]; contained of Water 8176 Meaſures, weighing 14643 lbs.

KIND OF FUEL USED.—[Pine Wood, Moderately dry, in Lengths of 6 Feet.]

GENERAL RESULTS OF THE EXPERIMENT.

	h.	min.
Time employed to make the Liquid boil, —	[3	40]
Fuel conſumed to make the Liquid boil, —	[800] lbs.	
	h.	min.
Time the liquid continued to boil, — —	2	43
Fuel added to keep the Liquid boiling, —	100 lbs.	

Quantity of the Liquid evaporated [not obſerved].

PRECISE RESULT.

With the Heat generated in the Combuſtion of 1 lb. of the Fuel,
[13.23 lbs.] of ice-cold Water made to boil;
or [339.80 lbs.] of boiling-hot Water kept boiling 1 hour.

Every thing in this Table, except ſuch figures and words as are printed between crotchets, is contained in the printed forms: Hence it is evident how much theſe Tables tend to diminiſh the trouble of regiſtering the reſults of Experiments of this kind, and alſo to prevent miſtakes.

The example I have here given is an account of an Experiment, in which a very large quantity of water, equal to 15,590lbs. Avoirdupois in weight, or 1866 wine gallons of 231 cubic inches each; but it is evident that theſe Tables anſwer equally well for the ſmall quantity contained by the ſmalleſt ſaucepan.

The height of the barometer is expreſſed in Paris inches; that of the thermometer in degrees of Fahrenheit's ſcale.—The other meaſures, as well of length as of capacity, are the common meaſures of the country (Bavaria); and the weight is expreſſed in Bavarian pounds, of which 100 make 123.84 lbs. Avoirdupois.

What is entered under the head of GENERAL RESULTS OF THE EXPERIMENT, requires no explanation; but what I have called the PRECISE RESULT muſt be explained.

Having frequent occaſion to compare the reſults of Experiments made at different times, and in different ſeaſons of the year, as the temperature of the water in the Boiler when the fire is lighted under it is ſeldom the ſame in any two Experiments, and as the boiling heat varies with the variations of the preſſure of the atmoſphere, or of the height of the mercury in the barometer, it became neceſſary to make proper allowances for theſe differences. This I thought could beſt be done by determining, by computation, from the number of degrees the water was *actually heated*, and the quantity of Fuel conſumed in heating it that number of degrees, how much Fuel would have been required to have heated it 180 degrees, or from the point of freezing to that of boiling water (the boiling point being taken equal to the temperature indicated by 212° of Fahrenheit's thermometer, which is the boiling point under the mean preſſure of the atmoſphere at the ſurface of the ſea): Then, by dividing the weight of the water uſed in the Experiment, (expreſſed in pounds,) by the weight of the Fuel expreſſed in pounds neceſſary to heat it 180 degrees, or from the temperature of freezing to that of boiling water; this gives the number of pounds of ice-cold water which (according to the reſult of the given Experiment) *might have been* made to boil,—with the heat generated in the combuſtion of 1 lb. of the Fuel, under the mean preſſure of the atmoſphere at the level of the ſurface of the ſea.

The city of Munich, where all the Experiments were made of which I am about to give an account, being ſituated almoſt in the centre of Germany, lies very high above the level of the ſea. The mean height of the mercury in the barometer is only about 28 Engliſh inches, conſequently water boils at Munich at a lower temperature than at London. The difference is even too conſiderable to be neglected, it amounts to 2½ degrees of Fahrenheit's ſcale,—being 209½ degrees at a medium at Munich, and 212 degrees in all places ſituated near the level of the ſea. To render the reſults of my experiments and computations more ſimple and more generally uſeful, I ſhall always make due allowance for this difference.

Having, from the actual reſult of each Experiment, made a computation on the principles here deſcribed, ſhowing what (for the want of a better expreſſion) I have called the *Preciſe Reſult* of the Experiment, it is evident that theſe computations ſhow very accurately the comparative merit of the mechanical arrangements, and management of the Fire in conducting the Experiments, in as far as relates to the Economy of Fuel; for the more ice-cold water that can be made to boil with the heat generated in the combuſtion of any given quantity (1 lb. for inſtance) of Fuel, the more perfect of courſe (other things being equal) muſt be the conſtruction of the fire-place.

Under the head of Preciſe Result I have ſometimes added another computation, ſhowing how much "*boiling-hot water*" might, according to the reſult of the given Experiment, be *kept boiling* "*one hour*" with the heat generated in the combuſtion of "1 lb. of the Fuel." Though I have called this a *Preciſe Reſult*, it is evident that in moſt caſes it cannot be conſidered as being very exact, owing to the difficulty of eſtimating the quantity of Fuel in the fire-place, which is *unconſumed at the moment when the water begins to boil.*

In the foregoing example in making this computation I ſuppoſed that, when the water began to boil, there was wood enough in the fire-place *unconſumed* to keep the water boiling 43 minutes, and that the wood added afterwards (100 lb.) kept the water boiling the remainder of the time it boiled, or juſt 2 hours.

In moſt caſes, however, to ſave trouble in making theſe computations, I have ſuppoſed that all the wood employed in making the water boil is entirely conſumed in that proceſs, and that all the heat expended in *keeping the water boiling* is furniſhed by the Fuel which is added *after the water had begun to boil.* This ſuppoſition is evidently erroneous; but as the computation in queſtion can at beſt give but an inaccurate and doubtful reſult, labour beſtowed on it would be thrown away: But imperfect as theſe rough eſtimates are, they will however in many caſes be found uſeful.

In giving an account of the following Experiments, I ſhall not place them exactly in the order in which they were made, but ſhall arrange them in ſuch a manner as I ſhall think beſt, in order that the information derived from their reſults may appear in a clear point of view.

For greater convenience in referring to them, I ſhall number them all; and as I have already given numbers to the four I mentioned in the Firſt Chapter of this Eſſay, I ſhall proceed in regular ſucceſſion with the reſt.

Experiment, No. 5.

The firſt kitchen of the Houſe of Induſtry at Munich has already been deſcribed in the Firſt Chapter of this Eſſay; and it was there mentioned, that the daily expence of Fuel in that kitchen, when food (peas-ſoup) was prepared for 1000 perſons, amounted to 300 lb. in weight of dry beech-wood. Now as each portion of ſoup conſiſted of 1 lb., this gives 0.3 of a pound of wood for each pound of ſoup.

Experiment, No. 6.

The firſt kitchen of the Houſe of Induſtry having been pulled down, it was afterwards rebuilt on a different principle. Inſtead of Copper Boilers, Iron Boilers of a hemiſpherical form were now uſed, and each of theſe Boilers had its own ſeparate cloſed fire-place. The Boiler being ſuſpended by its rim in the brick-work, and room being left for the flame to play all round it. The ſmoke went off into the chimney by an horizontal canal, 5 inches wide and 5 inches high, which was concealed in the maſs of brickwork, and which opened into the fire-place on the ſide oppoſite to the opening by which the fuel was introduced.

The fire was made on a flat iron grate placed directly under the Boiler, and diſtant from its bottom about twelve inches. The aſh-pit door was furniſhed with a regiſter; but there was no damper to the canal by which the ſmoke went off into the chimney, which was a very great defect. The opening into the fire-place was cloſed by an iron door. Each of theſe Iron Boilers weighed about 148 lbs. Avoirdupois, was 25½ Engliſh inches in diameter, and 14.935 inches deep, and contained 190½ lbs. Bavarian weight of water, equal to 235.91 lbs. Avoirdupois, or about 28¼ Engliſh wine-gallons.

From this account of the manner in which theſe Iron Boilers were fitted up, it is evident that the arrangement was not eſſentially different from that of kitchens for hoſpitals, as they are commonly conſtructed.

From Experiments made with care, and often repeated, I found that to prepare 89 portions (or 89 lbs. Bavarian weight) of peas-ſoup in one of theſe Boilers, 43 lbs. of *dry beech-wood* were required as Fuel, and that the proceſs laſted four hours and an half: This gives 0.483 of a pound of wood for each pound of the ſoup.

In the firſt arrangement of this kitchen, only 0.3 of a pound of wood was required to prepare 1 lb. of ſoup: Hence it appears that the kitchen had not been improved,—conſidered with a view to the Economy of Fuel,—by the alterations which had been made in it. This was what I expected; for the object I had in view in conſtructing this kitchen was not to ſave Fuel, but to find out how much of it is waſted in culinary proceſſes, as they are commonly performed on a large ſcale in hoſpitals and other inſtitutions of public charity.—Till I knew this, it was not in my power to eſtimate, with any degree of preciſion, the advantages of any improvements I might introduce in the conſtruction of kitchen fire-places.

To determine in how far the quantity of Fuel neceſſary in any given culinary proceſs depends on the form of *the fire-place*, (the Boiler and every other circumſtance remaining the ſame,) I made the following Experiments.

Experiments, No. 7 *and* No. 8.

Two of the Iron Boilers in the kitchen of the Houſe of Induſtry (which, as they were both caſt from the ſame model, were as near alike as poſſible) being choſen for this Experiment, one of them (No 8.) being taken out of the brick-work, its fire-place was altered and fitted up anew on improved principles. The grate was made circular and concave, and its diameter was reduced to 12 inches; the fire-place was made cylindrical above the grate, and only 12 inches in diameter; and the Boiler being ſeated on the top of the wall of this cylindrical fire-place, the flame paſſing through a ſmall opening on one ſide of the fire-place, at the top of it, made one complete turn about the Boiler before it was permitted to go off into the canal by which the ſmoke paſſed off into the chimney.

Though there was no damper in this canal, yet as its entrance or opening, where it joined the canal which went round the Boiler, was conſiderably reduced in ſize, this anſwered (though imperfectly) the purpoſe of a damper. This fire-place being completed, and a ſmall Fire having been kept up in it for ſeveral days to dry the maſonry, the Experiment was made by preparing the ſame quantity of the ſame kind of ſoup in this, and in a neighbouring boiler whoſe fire-place had not been altered.

The food cooked in each was 89 lbs. of Peas-ſoup; and the Experiment was begun and finiſhed in both Boilers at the ſame time.

The wood employed as Fuel was pine; and it had been thoroughly dried in an oven the day before it was uſed.

The boilers were both kept conſtantly covered with their double covers, except only when the Soup was ſtirred about to prevent its burning to the bottoms of the Boilers.

The reſult of this intereſting Experiment was as follows:

	Experiment No. 7. In the Boiler No. 1.	Experiment No. 8. In the Boiler No. 8, with the improved Fire-place.
Quantity of wood conſumed in cooking 89 lbs. Bavarian weight of Peas-ſoup - - - - - - -	37 lbs.	14 lbs.

Theſe Experiments were made on the 7th of November 1794. On repeating them the next day with pine-wood, which had not been previouſly dried in an oven, the reſult was as follows:

Experiments, No. 9 *and* No. 10.

	Experiment No. 9. In the Boiler No. 1.	Experiment No. 10. In the Boiler No. 8. with the improved Fire-place.
Quantity of wood conſumed in cooking 89 lbs. of Peas-ſoup - - -	39 lbs.	16 lbs.

The firſt remark I ſhall make on the reſults of theſe Experiments is the proof they afford, by comparing them with that which preceded them (No. 6.), of the important fact, that pine-wood affords more heat in its combuſtion than beech. This fact is the more extraordinary, as it is directly contrary to the opinion generally entertained on that ſubject; and it is the more important, as the price of pine-wood is, in moſt places, only about half as high as that of beech, when the quantities, *eſtimated by weight*, are equal.

In the Experiment No. 6. it was found, that 43 lbs. of dry beech-wood were neceſſary, when uſed as Fuel, to prepare 89 lbs. of Peas-ſoup. In the Experiment No. 7. the ſame proceſs was performed with 37 lbs. and in the Experiment No. 9.

with

with 39 lbs. of dry pine. But I ſhall have occaſion to treat this ſubject more at length in another place. In the mean time I would, however, juſt obſerve, that all my Experiments have uniformly tended to confirm the fact, that dry pine-wood affords more heat in combuſtion than dry beech. I have reaſon to think the difference is in fact greater than the Experiments before us indicate; but the *apparent* amount of it will always depend in a great meaſure on the circumſtances under which the Fuel is conſumed; or, in other words, on the conſtruction of the fire-place; and it is no ſmall advantage attending the fire-places I ſhall recommend, that they are ſo contrived as to increaſe, as much as it is poſſible, the ſuperiority of the moſt common and cheapeſt fire-wood over that which is more ſcarce and coſtly.

By comparing the reſults of theſe two ſets of Experiments (No. 7 and No. 8, No. 9 and No. 10.) an eſtimate may be made of the advantage of uſing *very dry-wood* for Fuel, inſtead of making uſe of wood that has been leſs thoroughly dried; but as I mean to take an opportunity of inveſtigating that matter alſo more carefully hereafter, I ſhall not at preſent enlarge on it farther than juſt to obſerve, that as the wood, which was dried in an oven, was weighed for uſe after it had been dried, and as it certainly weighed more before it was put into the oven, the real ſaving ariſing from uſing it in this dried ſtate is not ſo great as the difference in the weights of the quantities of wood uſed in the two

Experiments. To eſtimate that ſaving with preciſion, the wood ſhould be weighed before it is dried, or in the ſame ſtate in which the other parcel of wood, which is uſed without being dried, is weighed.

But to proceed to the principal object I had in view in theſe Experiments;—the determination of the effects of the difference in the conſtruction of the two fire-places;—the difference in the quantity of Fuel expended in the two fire-places in performing the ſame proceſs, ſhows, in a manner which does not ſtand in need of any illuſtration, how much had been gained by the improvements which had been introduced.

Conceiving it to be an object of great importance to aſcertain by actual experiment, and with as much preciſion as poſſible, the real amount of the advantages, in regard to the Economy of Fuel, that may be derived from improvements in the forms of fire-places, I did not content myſelf with improving from time to time the kitchens I had conſtructed, but I took pains to determine how much I had gained by each alteration that was made. This was neceſſary, not only to furniſh myſelf with more forcible arguments to induce others to adopt my improvements, but alſo to ſatisfy myſelf with regard to the progreſs I made in my inveſtigations.

In the firſt arrangement of the kitchen of the Military Academy, the Boilers were ſuſpended by their rims in the brick-work in ſuch a manner that the flame could paſs freely all round them, and the ſmoke went off in horizontal canals which led to the chimney, but which were not furniſhed with dampers.

The Fire was made on a flat ſquare iron-grate; and the internal diameter of the fire-place was 2 or 3 inches larger than the diameter of the Boiler which belonged to it. The bottom of the Boiler was from 6 to 10, or 12 inches (according to its ſize) above the level of the grate; and the door of the opening into the fire-place, by which the Fuel was introduced, was kept conſtantly cloſed. The aſh-pit door was furniſhed with a regiſter, and the Boilers were all furniſhed with double covers.

Having, in conſequence of the progreſs I had made in my inquiries reſpecting the management of Heat and the Economy of Fuel, come to a reſolution to pull down this kitchen, and rebuild it on an improved principle; previous to its being demoliſhed, I made ſeveral very accurate Experiments to determine the real expence of Fuel in the fire-places as they *then exiſted*, with all their faults; and when the new arrangement of the kitchen was completed, I repeated theſe Experiments *with the ſame boilers*; and by comparing the reſults of theſe two ſets of Experiments, I was able to eſtimate with great preciſion the real amount of the ſaving of time as well as of Fuel,—which were derived from the improvements I had introduced.

After all that has been ſaid (and perhaps already too often repeated in different parts of this Eſſay) on the conſtruction of fire-places, my reader will be able to form a clear and juſt idea of the conſtruction of thoſe of which I am now ſpeaking, (thoſe of the kitchen of the Military Academy, in its *preſent* improved ſtate,) when he is told that the Fire burns on a circular concave iron grate, about half the diameter of the circular Boiler which belongs to the fire-place;—that the fire-place, properly ſo called, is a cylindrical cavity in the ſolid brick-work which ſupports the Boiler, equal in diameter to the circular grate, and from ſix to ten inches high, more or leſs according to the ſize of the Boiler;—that the Boiler is *ſet down* on the top of the circular wall which forms this fire-place, a ſmall opening, from three to four or five inches in length taken horizontally, and about two or three inches high, being left on one ſide of this wall at the top of it, in order that the flame which burns up under the middle of the bottom of the Boiler may afterwards paſs round (in a ſpiral canal conſtructed for that purpoſe) under that part of the bottom of the Boiler which lies *without* the top of the wall of the fire-place on which the Boiler repoſes. The flame having made one complete turn *under* the Boiler in this ſpiral canal, it riſes upwards, and going once *round the ſides of the Boiler*, goes off by an horizontal canal, furniſhed with a damper, into the chimney.

In order that the top of the circular wall of the fire-place on which the Boiler is ſeated, may not cover too much of the bottom of the Boiler, its thickneſs is ſuddenly reduced *in that part* (that is to ſay juſt where it touches the Boiler) to about half an inch.

The opening by which the Fuel is introduced into the fire-place, is a conical hole in a piece of fire-ſtone, which hole is cloſed by a fit ſtopper made of the ſame kind of ſtone. The aſh-pit door and its regiſter are finiſhed with ſo much nicety, that when they are quite cloſed the Fire almoſt inſtantaneouſly goes out.

The dimenſions of the Boiler, in which the Experiments of which I am about to give an account were made, are as follows :

Diameter	above	14.935	Inches Engliſh meaſure.
	below	13.39	
Depth		14.52	

It weighs 37 lbs. Avoirdupois ; and it contains, when quite full, about 73 lbs. Avoirdupois, equal to 8¾ gallons (wine meaſure) of water.

In two Experiments with this Boiler, which were both made by myſelf, and in which attention was paid to every circumſtance that could tend to render them perfect, the reſults were as follows :

	Experiment No. 11. The firſt Fire-place.	Experiment No. 12. The improved Fire-place.
Quantity of Water in the Boiler, in *Bavarian pounds* - - - -	43.63 lbs.	43.63 lbs.
Temperature of the water in the Boiler at the beginning of the Experiment - - - - - -	59°	60°
Time employed in making the water boil - - - - - - -	67 min.	30 min.
Wood conſumed in making the water boil, in *Bavarian pounds* -	9 lbs.	3 lbs.
Time the water continued boiling - - - - - - - - - -	2 hrs. 2 min.	3 hours
Wood added to keep the water boiling - - - - - - - -	5 lbs.	2½ lbs.
Kind of wood uſed - - - -	Pine	Pine
Preciſe Reſults.		
Ice-cold water heated 180 degrees, or made to boil, with 1 lb. of wood - - - - - - - - -	4.02 lbs.	11.93 lbs.
Boiling-hot water kept boiling 1 hour, with 1 lb. of wood -	17.74 lbs.	52.36 lbs.

The following Experiments were made with two Copper Boilers, (No. 1 and No. 2.) nearly of the ſame dimenſions, in the kitchen of the Military Academy at Munich, in the preſent improved ſtate of that kitchen. Theſe Boilers are round and deep; and weigh each about 62 lbs. Avoirdupois. They belonged originally to the kitchen of the Houſe of Induſtry, being two of the eight Boilers which, in the firſt arrangement of that kitchen, were heated by the ſame Fire.

Their exact dimenſions, meaſured in Engliſh inches, are as follows:

		The Boiler No. 1.	The Boiler No. 2.
		Inches.	Inches.
Diameter	above —	22.66	22.66
	below —	19.82	20.85
Depth — — —		24.72	22.04

At the beginning of each of the following Experiments, each of theſe Boilers contained juſt 95 meaſures (or *Bavarian maaſſe*) of water, weighing 187 lbs. Bavarian weight, (equal to 232.58 lbs. Avoirdupois,) or a trifle leſs than 28 gallons.

The grate on which the Fire was made under each of theſe Boilers is circular and concave, and 11 inches in diameter; and their fire-places are in all reſpects ſimilar to that juſt deſcribed. (Experiment No. 11.) Both Boilers are furniſhed with double covers.

The Experiments made with the Boiler No. 1, and their reſults, were as follows:

	Experiment No. 13.	Experiment No. 14.	Experiment No. 15.	Experiment No. 16.
Quantity of water in the Boiler in the beginning of the Experiment - -	lbs. 187	lbs. 187	lbs. 187	lbs. 187
Temperature of the water in the Boiler at the beginning of the Experiment -	61°	59°	64°	55½°
Time employed in making the water boil - - - -	min. 78	min. 61	min. 61	min. 62
Wood conſumed in making the water boil - - - -	lbs. 12	lbs. 11	lbs. 9	lbs. 8
Time the water continued to boil - -	min. 17	min. 28	min. 6	h. min. 2 19
Quantity of Fuel added to keep it boiling this time - -	—	—	—	lbs. 4
Kind of wood uſed as Fuel - - - -	Beech	Beech	Pine	Pine
Preciſe Reſults of the Experiments.				
Ice-cold water heated 180°, or made to boil, with the heat generated in the combuſtion of 1 lb. of the Fuel - -	lbs. 12.89	lbs. 14.15	lbs. 16.89	lbs. 20
Boiling water kept boiling one hour, with the heat generated in the combuſtion of 1 lb. of the wood - - -	—	—	—	lbs. 108.40

All the foregoing Experiments were made on the ſame day, (the 13th of October 1794,) and in the ſame order in which they are numbered.

The following are the reſults of the Experiments made with the Boiler No. 2.

	Experim. No. 17	Experim. No. 18.	Experim. No. 19.	Experim. No. 20.	Experim. No. 21.
Quantity of water in the Boiler at the beginning of the Experiment, in *Bavarian pounds* —	lbs. 187	lbs. 187	lbs. 187	lbs. 187	lbs. 187
Temperature of the water in the Boiler at the beginning of the Experiment — —	61°	58°	60°	55°	212°
Time employed in making the water boil —	min. 75	min. 55	min. 57	min. 60	—
Wood conſumed in making the water boil —	lbs. 11	lbs. 11	lbs. 9	lbs. 8	—
Time the water continued to boil — —	min. 21	min. 17	min. 8	h. min. 2 29	h. min. 1 10
Wood added to keep the water boiling — —	lb. 1	—	—	lbs. 3½	lbs. 1½
Kind of wood uſed —	Beech	Beech	Pine	Pine	Beech
Preciſe Reſults.					
Ice-cold water heated 180°, or made to boil, with 1 lb. of wood —	lbs. 13.92	lbs. 14.33	lbs. 17.59	lbs. 20.10	—
Boiling-hot water kept boiling one hour with 1 lb. of wood — —	—	—	—	lbs. 132.68	lbs. 145.44

This ſet of Experiments was made at the ſame time with the foregoing ſet, namely, on the 13th October 1794, and they were made in the order in which they are here regiſtered. In the laſt but one, (No. 20.) the Economy of Fuel in the proceſs of heating water was carried farther than in any other Experiment I have ever made.

In the following Experiments, which were made in a large Copper Boiler fitted up on my moſt improved principles, belonging to the kitchen of the Houſe of Induſtry, the Economy of Fuel was carried nearly as far.

This Boiler, which is circular, is 42½ Engliſh inches in diameter above; 42.17 inches in diameter below; and 18.54 inches deep. It weighs 78½ lbs. Avoirdupois; and contains, when quite full, 714 lbs. *Bavarian weight* (=884 lbs. Avoirdupois, or 106 gallons) of water, at the temperature of 55°.

It is ſurrounded above by a wooden ring about two inches in thickneſs, into which it is fitted; and in this ring, in a groove about ¾ of an inch deep, is fitted a circular wooden flat cover; this cover is formed in three pieces, united by iron hinges; and one of theſe pieces being faſtened down by hooks to the Boiler, the other two are ſo contrived as to be folded back upon it occaſionally. From the upper ſurface of the part of the cover which is faſtened down on the Boiler, a tin tube two inches in diameter, furniſhed with a damper, is fixed, by which the ſteam is carried off into a narrow wooden tube, which conducts it through an opening in the roof of the houſe into the open air.

To prevent ſtill more effectually the eſcape of the Heat through the wooden cover of the Boiler, the upper ſurface of it is protected from the cold atmoſphere by a thick circular blanket covered on both ſides by ſtrong canvas, which is occaſionally thrown over it.

Though

Though the diameter of this Boiler below is more than 40 inches, the diameter of its fire-place (which is juſt under its centre) is only 11 inches; but as the flame makes two complete turns under the bottom of the Boiler in a ſpiral canal, and one turn round it, the time required to heat it is not ſo great as, from the ſmallneſs of its fire-place, might have been expected.

It has ever been, and ſtill continues to be, the decided favourite of the cook-maids.

The wood uſed as Fuel in the following Experiment was pine moderately dried. The billets were ſix inches long, and from one to two inches in diameter.

The following Table ſhows the reſults of five Experiments that were made with this Boiler by myſelf, juſt after it was fitted up:

	Experim. No. 22.	Experim. No. 23.	Experim. No. 24.	Experim. No. 25.	Experim. No. 26.
Quantity of water in the Boiler, in Bavarian pounds — — —	lbs. 508	lbs. 127	lbs. 254	lbs. 508	lbs. 508
Temperature of the water at the beginning of the Experiment —	48°	48°	96°	48°	48°
Time required to make the water boil — —	h. min. 2 4	min. 51	h. min. 1 15	h. min. 2 35	h. min. 3 1
Fuel employed to make the water boil — —	lbs. $24\frac{1}{2}$	lbs. $8\frac{1}{4}$	lbs. $12\frac{7}{8}$	lbs. 25	lbs. 24
Time the water continued boiling — —	h. 3	—	—	h. 3	—
Fuel added to keep the water boiling — —	lbs. $6\frac{12}{32}$	—	—	lbs. $4\frac{1}{2}$	—
Precise Results of *the Experiments.*					
With the heat generated in the combuſtion of 1 lb. of the Fuel,					
Ice-cold water heated 180°, or made to boil	lbs. 18.87	lbs. 12.74	lbs. 12.69	lbs. 17.48	lbs. 19.01
Or boiling-hot water kept boiling one hour	236.61	—	—	338.66	—

Without ſtopping to make any obſervations on the reſults of theſe Experiments, (though they afford matter for ſeveral of an intereſting nature,) I ſhall proceed to give a brief account of another ſet of Experiments, on a much larger ſcale, which were made in the Copper Boiler of a Brewery belonging to the Elector.

This Boiler, which is rectangular, is ten feet long, eight feet wide, and four feet deep, *Bavarian meaſure* *, and contains 8176 *Bavarian maaſſe*, or meaſures, equal to 1866 gallons wine meaſure. On examining this Boiler, I found its fire-place was conſtructed on very bad principles; and on inquiring reſpecting the quantity of fire-wood conſumed in it, I found the waſte of Fuel to be very great.

This Brewery is uſed for making ſmall *white* beer, (as from its pale colour it is called,) from malt made of wheat; and as it is worked all the year round, the expence of Fuel was very great, and the economy of it an object of conſiderable importance.

The quantity of fire-wood (pine) that had at an average been conſumed daily in this Brewery was rather more than four Bavarian *claſters*, or cords. On altering the fire-place of this Brewery, and putting a (wooden) cover to the Boiler, I reduced this expence to leſs than 1½ claſters.

In the new fire-place which I cauſed to be conſtructed for this Boiler, the cavity under the Boiler is divided into three flues, by thin brick walls which run in the direction of the length of the Boiler. The middle flue, which is twice as wide as one of the ſide flues, is occupied by the burning Fuel, and is furniſhed with a grate 20 inches wide, and 36 inches long; and the opening by which the Fuel is introduced into the fire-place is cloſed by two iron doors, placed one behind the other, at the diſtance of eight inches. The grate, which is placed at the hither end of the fire-place, is horizontal, and it is ſituated about twenty inches below the bottom of the Boiler. The air which ſerves to feed the Fire, is let in under the grate through a regiſter in the aſh-pit door.

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* 100 Bavarian inches are equal to 95¾ inches Engliſh meaſure.

When the double doors which cloſe the entrance into the fire-place are ſhut, the flame of the burning Fuel firſt riſes perpendicularly againſt the bottom of the Boiler; it then paſſes along to the farther end of the (middle) flue, which conſtitutes the fire-place, where it ſeparates, and returns in the two ſide flues; it then riſes up into two horizontal flues (one ſituated over the other) which go all round the Boiler; and having made the circuit of the Boiler, it goes off into ſeparate canals (furniſhed with dampers) into the chimney.

Though the Figures 17 and 18, Plate III. are not drawings from the fire-place I am now deſcribing, but of another which I ſhall ſoon have occaſion to deſcribe, yet an inſpection of theſe figures will be found uſeful in forming an idea of the principles on which the fire-place in queſtion was conſtructed, and on that account I ſhall occaſionally refer to them.

The burning Fuel being confined within a narrow compaſs,—being well ſupplied with freſh air,—and being ſurrounded on all ſides by thin walls of brick, (which are non-conductors,) the heat of the fire is moſt intenſe, and the combuſtion of the Fuel of courſe very complete. The flame, which is clear and vivid in the higheſt degree, and perfectly unmixed with ſmoke, runs rapidly along the bottom of the Boiler, (which forms the top of the flues,) and from the reſiſtance it meets with in its paſſage, from friction, and from the number of turns it is obliged to make, it is thrown into innumerable eddies and whirlpools, and really affords a moſt entertaining ſpectacle.

That I might be able to enjoy at my eaſe this amuſing ſight, I cauſed a glaſs window to be made in the front wall of the fire-place, through which I could look into the Fire when the fire-place doors were ſhut; and I was well paid for the trouble and the trifling expence I had in getting it executed.

Some may be tempted to ſmile at what they may think a childiſh invention; but there are many others, I am confident, and among theſe many grave philoſophers, who would have been very glad to have ſhared my amuſement.

The window of which I am ſpeaking is circular, and only ſix inches in diameter; but as the hole in the wall is conical, and much larger within than without, the field of this window (if I may uſe the expreſſion) is ſufficiently large to afford a good view of what paſſes in the fire-place.

This conical hole is repreſented in the Figures 18 and 21, by dotted lines. It is ſituated on the left hand of the entrance into the fire-place. Into the opening of the hole in the wall, on the outſide of it, is fixed a ſhort tube of copper, (about ſix inches in diameter, and four inches long,) and in this tube another ſhort *moveable* tube is fitted, one end of which is cloſed by the circular plate of glaſs which conſtitutes the window. As the wall of the fire-place in front is thick, this pane of glaſs is at a conſiderable diſtance from the burning Fuel, and as there is no draught through the hole in the wall, the glaſs does not grow very hot.

I have been the more particular in my deſcription of this little invention, as I think it may be uſeful: There are many caſes in which it would be very advantageous to know exactly what is going on in a cloſed fire-place; and this never can be known by opening the door; for the inſtant the door is opened, the cold air ruſhing with impetuoſity into the fire-place, deranges entirely the whole economy of the Fire: Beſides this, it is frequently very diſadvantageous to the proceſs which is going on, to open the door of a fire-place; and it is always attended with a certain loſs of heat, and conſequently ſhould as much as poſſible be avoided.

I intimated that the window I have been deſcribing afforded me amuſement;—it did ſtill more,—it afforded me much uſeful information;—it gave me an opportunity of *obſerving* the various internal motions into which flame may, by proper management of the machinery of a fire-place, be thrown; and of eſtimating, with ſome degree of preciſion, their different effects. In ſhort, it made me better acquainted with the ſubject which had ſo long engaged my attention—(Fire);—and with regard to *that* ſubject, nothing ſurely that is new can be unintereſting. But to return to the Brewery:—To the top of the Boiler was fitted a curb of oak timber: The four ſtraight beams of which this curb was conſtructed are each about 7 inches thick, and 15 inches wide; and the upper part of the Boiler is faſtened by large copper nails to the inſide of the ſquare frame formed by theſe four beams. From the top of this curb is raiſed a wooden building, like the roof of a houſe with a double ſlant or bevel, which ſerves as a cover to the Boiler. This Building, the ſides of which are about three feet high inwards, and the top of which is covered in by a very flat roof, ſlanting on every ſide from the centre,—is conſtructed of a light frame-work of timber, (four-inch deal joiſts,) which is covered within as well as without with thin deal boards, which are rabbetted into each other at their edges, to render the cover which this little edifice forms for the Boiler as tight as poſſible.

From the top of this cover, an open wooden tube, (*m*, Fig. 17.) about 12 inches in diameter, riſes up perpendicularly, and going through the roof of the Brewhouſe, ends in the open air. This tube, which is furniſhed with a wooden damper, is intended to carry off the ſteam.

On the ſide of this cover next the maſhing-tub, as alſo on that oppoſite to it, by which the wort runs off into the coolers, there are large folding wooden doors, (*i* and *k*, Fig. 17.) which are occaſionally lifted up by means of ropes which paſs over pullies faſtened to the ceiling of the Brewhouſe.

There are likewiſe two glaſs windows (ſee Fig. 17.) in two oppoſite ſides of the cover, through which, as ſoon as in conſequence of the boiling of the liquid the ſteam becomes tranſparent and *inviſible*, (which happens in a very few minutes after the liquid has begun to boil,) the contents of the Boiler may be diſtinctly ſeen and examined.

Whenever there is occaſion during the boiling to open either a door or a window of the cover, it is neceſſary to begin by opening the damper of the ſteam-chimney, otherwiſe the hot ſteam, ruſhing out with violence, would expoſe the by-ſtanders to the danger of being ſcalded; but when the damper of the ſteam-chimney is open, no ſteam comes into the Brewhouſe, though a door or window of the cover be wide open.

Another ſimilar precaution is ſometimes neceſſary in opening the door of the fire-place, which it may be uſeful to mention.—When the dampers in the canals by which the ſmoke goes off into the chimney are nearly cloſed, (which muſt frequently be done to confine and economiſe the heat,) if, without altering the damper, or the regiſter in the aſh-pit door, the fire-place door be ſuddenly opened, it will frequently happen that ſmoke, and ſometimes flame, will ruſh out of the fire-place by this paſſage. This accident may be eaſily and effectually prevented, either by opening the damper, or by cloſing the regiſter of the aſh-pit door, the moment before the fire-place door is opened.—This precaution ſhould be attended to in all fire-places of all dimenſions, conſtructed on the principles I have recommended.

To economiſe the time and the *patience* of my reader as far as it is poſſible, without ſuppreſſing any thing eſſential relating to the ſubject under conſideration, I ſhall give him, in a very ſmall compaſs, the general reſults of a ſet of Experiments which coſt me more labour (or at leaſt more *time*) than it would coſt him to read all the Eſſays I have ever written. I believe I am ſometimes too prolix for the taſte of the age,—but it ſhould be remembered that the ſubjects I have undertaken to inveſtigate are by no means indifferent to me;—that I conceive them to be intimately connected with the comforts and enjoyments of mankind;—and that a habit of revolving them in my mind, and reflecting on their extenſive uſefulneſs, has awakened my enthuſiaſm, and rendered it quite impoſſible for me to treat them with cold indifference, however indifferent or tireſome they may appear to thoſe who have not been accuſtomed to view them in the ſame light.

I have already given an account, in all its various details, of one Experiment which was made (on the 15th of April 1795) with the Boiler we have juſt been deſcribing (ſee page 78). I ſhall now recapitulate the general reſults of that Experiment, and compare them with the mean reſults of two other like Experiments made with the ſame Boiler.

	Experiment No. 27.	Experiment No. 28.
Quantity of water in the boiler -	12,508 lbs.	12,508 lbs.
Temperature of the water in the Boiler at the beginning of the Experiment - - - - -	60°	58°
Time required to make the water boil - - - - - - - -	3 h. 40 min.	3 h. 48 min.
Fuel employed to make the water boil - - - - - - -	800 lbs.	825 lbs.
Time the water continued boiling - - - - - - - -	2h. 43 min.	—
Fuel added to keep the water boiling - - - - - - -	100 lbs.	—
Kind of Fuel uſed - - - -	Pine-wood	Pine-wood
PRECISE RESULTS of the Experiments.		
Quantity of *ice-cold water* which might be heated 180°, or made to boil, with the heat generated in the combuſtion of 1 *lb. of the Fuel* - - - - - -	12.06 lbs.	12.70 lbs.
TIME in which, according to the reſult of the Experiment, *ice-cold water* might (at Munich) *be made to boil* with the given proportion of Fuel - - -	4 h. 20 min.	4 h. 20 min.
Quantity of *boiling-hot water kept boiling one hour* with the heat generated in the combuſtion of 1 lb. of the Fuel -	339.80 lbs.	—

On comparing the reſults of theſe Experiments with thoſe made in the boilers of the kitchens of the Houſe of Induſtry and Military Academy, I was led to imagine that either the Boiler, or the fire-place of the Brewery, or both, were capable of great improvement; for in ſome of the Experiments with theſe ſmall kitchen Boilers, the Economy of Fuel had been carried ſo far, that with the heat generated in the combuſtion of 1 lb. of pine-wood, it appeared that 20 lbs. of ice-cold water might have been made to boil; but here, though the machinery was on a ſcale ſo much larger, (and I had concluded, too raſhly indeed, as will be ſhown hereafter, that the larger the Boiler the greater is of courſe the Economy of Fuel,)—the reſults of theſe Experiments indicated, that not quite 13 lbs. of ice-cold water could have been made to boil with the heat furniſhed in the combuſtion of 1 lb. of the wood.

The Experiments, No. 22, No. 25, and No. 26, which were made with the largeſt of my kitchen Boilers, had, it is true, afforded grounds to ſuſpect that, beyond certain limits, an increaſe of ſize in a Boiler does not tend to diminiſh the expence of Fuel in the proceſs of heating water; yet, as all my other Experiments had tended to confirm me in the opinion I had, at an early period, imbibed on that ſubject, I was diſpoſed to ſuſpect any other cauſe than the true one, of having been inſtrumental in producing the unexpected appearances I obſerved.

I was much diſappointed, I confeſs, at finding that the Brewhouſe Boiler, notwithſtanding all the pains I had taken to fit up its fire-place in the moſt perfect manner, and notwithſtanding its enormous dimenſions, when compared with the Boilers I had hitherto uſed in my Experiments, ſo far from anſwering my expectations, actually required conſiderably more Fuel, in proportion to its contents, than any other Boiler fitted up on the ſame principles, which was not *one fiftieth* part of its ſize.

This unexpected reſult puzzled me;—and I muſt own that it vexed me, though I ought perhaps to be aſhamed of my weakneſs;—but it did not diſcourage me. Finding on examining the Boiler, that its bottom was very thick, compared with the thickneſs of the ſheet copper of which my kitchen Boilers were conſtructed, it occurred to me that poſſibly *that* might be the cauſe, or at leaſt *one of the cauſes*, which had made the conſumption of Fuel ſo much greater than I expected; and as there was another Brewhouſe in the neighbourhood belonging to the Elector, which, luckily for me, ſtood in need of a new boiler, I availed myſelf of that opportunity to make an Experiment, which not only decided the point in queſtion, but alſo eſtabliſhed a new fact with regard to heat, which I conceive to be of conſiderable importance.

Having obtained the Elector's permiſſion to arrange the ſecond Brewhouſe as I ſhould think beſt, I determined to ſpare no pains to render it as perfect as poſſible in all reſpects, and particularly in every-thing relating to the Economy of Fuel. As in brewing, in the manner that buſineſs is carried on in Bavaria, where the whole proceſs, in as far as Fire is employed in it, is begun and finiſhed in the courſe of a day, *the ſaving of time*, in heating the water and boiling the wort, is an object of almoſt as much importance as that of economizing Fuel, and conſequently demanded particular attention.

The means I uſed for the attainment of both theſe objects will be evident from the following deſcription of the Boiler, and its fire-place, which I cauſed to be conſtructed, and which are repreſented in all their details in the Plates III, IV, and V.

This Boiler is 12 (Bavarian) feet long, 10 feet wide, and only 2 feet deep. The ſheet copper of which it is made is uncommonly thin for a Boiler of ſuch large dimenſions, being at a medium leſs than *one tenth* of an Engliſh inch in thickneſs. This Boiler, when finiſhed, weighed no more than 674 lbs. Bavarian weight, equal to 834¼ lbs. Avoirdupois, excluſive of 64 lbs. of copper nails uſed in riveting the ſheets of copper together.

The top of the Boiler is ſurrounded by a ſtrong curb (*a*, *b*, Fig. 17.) of oak timber, to which it is attached by ſtrong copper nails, and over the Boiler is built a roof, or ſtanding cover, (ſee fig. 17.) ſimilar in all reſpects to that already deſcribed. The bottom of the Boiler is flat, and repoſes horizontally on the top of the thin brick walls by which the fire-place is divided into flues. (See Fig. 18.)—Theſe flues do not run in the direction of the length of the Boiler, but from one ſide of it to the other;—conſequently the door of the fire-place is in the middle of one ſide of the Boiler.

The ſheets of Copper, of which the bottom of the Boiler was conſtructed, run in the direction of the flues; and they are juſt ſo wide that their ſeams or joinings (where they are united to each other by their ſides) repoſe on the walls of the flues, except only in the middle flue, which, being about twice as wide as the others, one ſeam was neceſſarily left unſupported, at leaſt a conſiderable part of its length.—The ſheets of copper uſed in conſtructing this part of the bottom of the Boiler are rather thicker and ſtronger than the reſt: They are juſt 0.118 of an Engliſh inch in thickneſs.

The fire is made under this Boiler in the middle flue, which, as I have juſt obſerved, is a little more than twice as wide as one of the other flues. There are *five* flues under the Boiler; namely, one in the middle 44 inches wide, above, in the clear, (which conſtitutes the fire-place,)—and *two* on each ſide of it, in which the flame circulates; one 20 inches wide, and the other 19 inches wide.

The ſide flues are each 14½ inches deep;—but as the walls which ſeparate them are much thicker below than above, where the bottom of the Boiler repoſes on them, the width of theſe flues below is only 13 inches.—The walls of theſe flues are ſhown by dotted lines in Fig. 17.

The walls which ſeparate the flues do not run quite from one ſide of the Boiler to the other; an opening being left at one end of each of them equal to the width of one of the narrow flues for the paſſage of the flame from one flue into another, without its going from under the Boiler.

The Fire being made (on a circular grate) in the middle flue (ſee Fig. 18.), the flame paſſes on in this flue to its farther end, and then, dividing to the right and left, comes forward in the two adjoining ſide-flues. Having arrived at the wall which ſupports the front of the Boiler, it turns again to the right and left; and, entering the two outſide flues, returns in them to the back of the Boiler. Here it went out (before the fire-place was altered) at two openings left for that purpoſe in the wall which ſupports the back part of the Boiler, and the two currents of flame uniting, entered a canal 7 inches wide, and 16 inches high, which goes all round the outſide of the Boiler. (See Fig. 20.) Having made the circuit of the Boiler, it went off by a canal (furniſhed with a damper) into the chimney.

From this deſcription of the fire-place, it appears that the flame and ſmoke generated in the combuſtion of the Fuel in paſſing through thoſe different flues, made a circuit of above 70 feet, in contact with the ſurface of the Boiler, before they were permitted to eſcape into the chimney. This I thought muſt be ſufficient to give theſe hot fluids an opportunity of communicating to the Boiler all the heat they could part with, notwithſtanding the difficulties which attend their getting rid of it. And I concluded that the communication of their heat to the Boiler would be much facilitated and expedited by the various eddies and whirlpools produced in the flame in conſequence of the number of abrupt turns and changes of direction it was obliged to make in paſſing under and round the Boiler.

As the Experiments which have been made with this Boiler were conducted throughout with the utmoſt care and attention, and as their reſults are both curious and important in ſeveral reſpects, I have thought them deſerving of being made known to the Public in all their details.

An Account of three Experiments made at Munich the 10th of October 1796, with the new Boiler in the Brewery called *Neuheuſel*, belonging to HIS MOST SERENE HIGHNESS the ELECTOR.—The weather being fair: The barometer ſtanding at 28 Engliſh inches, and Fahrenheit's thermometer at 36°.

Dimenſions of the Boiler, in Engliſh meaſure, as found by actual admeaſurement,	Length	11	feet	6.02 inches,
	Width	9	—	7.723 ——
	Depth	2	—	0.205 ——

Contents of the Boiler, when quite full to the brim, 14,163 lbs. *Bavarian weight* of water, at the temperature of 55°, equal to 17,540 lbs. Avoirdupois, or 2099 wine gallons.

The Boiler actually contained of water, in the beginning of each of the two following experiments, - - - } *in Bavarian weight* ——— 8120 lbs.

equal to 10.056 lbs. Avoirdupois, or nearly 1204 wine gallons.

The wood uſed in this and the following Experiments was *Pine*, which had been moderately ſeaſoned, and the billets were 3 feet 4¼ inches, Engliſh meaſure, in length.

FIRST

FIRST EXPERIMENT WITH THE NEW BOILER.

Experiment, No. 29.

Time.		Fire-wood put into the Fire-place.		Temperature of the water in the Boiler.
Hours.	Min.	Number of Billets.	Quantity in weight. lbs.	In degrees of Fahrenheit's Therm.
11	31 A. M.	10	50	50°
—	46 —	15	25	54
12	0 —	5	25	64
—	10 P.M.	5	25	67
—	36 —	—	—	85
—	40 —	4	25	—
—	53 —	5	25	96
1	12 —	7	25	105
—	21 —	10	50	110
—	46 —	10	50	129
—	58 —	40	50	—
2	17 —	46	50	156
—	29 —	—	—	164
—	34 —	10	50	—
—	41 —	—	—	173
—	49 —	—	—	180
—	58 —	40	50	185
3	15 —	12	50	197
—	26 —	20	25	205
3	35 —	—	—	the water boiled.

Time employed, } 4 h. 4 min. Wood conſumed, 575 lbs.

The boiling water being let off, and it being replaced immediately with cold water, the Experiment was repeated as follows:

Experiment, No. 30.

Time.		Quantity of Fire-wood put into the Fire-place.		Temperature of the water in the Boiler.
Hours.	Min.	Number of Billets.	Quantity in weight. lbs.	In dgrees of Fahrenheit's Therm.
4	41 P.M.	40	50	60°
—	50 —	40	50	72
5	4 —	10	50	86
—	16 —	10	50	99½
—	29 —	10	50	114
—	42 —	10	50	126
—	56 —	40	50	142
6	10 —	40	50	157
—	24 —	40	50	—
—	28 —	—	—	172
—	40 — a	40	50	—
—	42½ —	—	—	185½
—	53 —	40	50	—
—	55 —	—	—	198
7	2 —	—	—	205
7	7 —	—	—	the water boiled.

Time employed, } 2 h. 26 min, Wood conſumed, 550lbs.

This boiling water being let off, the Boiler was again filled (immediately) with cold water; and in this third Experiment the quantity of water was increaſed to 11,368 lbs. *Bavarian weight*,—equal to 14,078 lbs. Avoirdupois, or 1685 wine gallons.

The reſults of this Experiment were as follows:

Experiment, No. 31.

Time.			Quantity of Fire-wood put into the Fire-place.		Temperature of the water in the Boiler.
Hours.	Min.		Number of Billets.	Quantity in weight. lbs.	In degrees of Fahrenheit's Therm.
8	51	P.M.	80	100	65½°
9	7	—	40	50	79°
—	21	—	40	50	90
—	44	—	40	50	107
—	57	—	40	50	118
10	14	—	40	50	130
—	28	—	40	50	140
—	45	—	40	50	155
11	—	—	40	50	165
—	15	—	40	50	175
—	30	—	40	50	182
—	45	—	40	50	200
11	58	—	—	—	the water boiled.

Time employed, } 3 h. 7. min. Wood conſumed, 650 lbs.

The following Table will ſhow the reſults of theſe three Experiments in a clear and ſatisfactory manner:

	Experiment No. 29.	Experiment No. 30.	Experiment No. 31.
Quantity of water in the Boiler at the beginning of the Experiment, in *Bavarian pounds* - - - - -	8120lbs.	8120lbs.	11,368lbs.
Temperature of the water at the beginning of the Experiment - -	50°	60°	65½°
Time employed in making the water boil -	4 h. 4 min.	2 h. 26 min.	3 h. 7 min.
Fuel (Pine wood) conſumed in making the water boil, in *Bavarian pounds* - - - - -	575lbs.	550lbs.	650lbs.
Preciſe Reſults of the Experiments.			
Quantity of Ice-cold water which might have been heated 180 degrees, or made to boil with the heat generated in the combuſtion of 1lb. of the Fuel - -	12.54lbs.	12,28lbs.	14.59lbs.
Time in which, according to the reſult of the Experiment, ice-cold water might be made to boil at Munich with the given proportion of Fuel - - - - - -	4 h. 31 min.	2 h. 59 min.	3 h. 35 min.

I was ſurpriſed, when I compared the reſults of theſe Experiments with thoſe made in the other

Brewhouſe, to find how little in appearance I had gained by the alterations I had introduced; on a more careful examination of the matter, however, I found that I had gained much more than I at firſt imagined, both in reſpect to the Economy of Fuel, and to that of Time. The amount of theſe advantages will appear from the following compariſon of the mean reſult of theſe two ſets of Experiments;

	Preciſe Reſults of the foregoing Experiments.		
	Quantity of ice-cold water made to boil with 1 lb. of the Fuel.	Time required to make ice-cold water boil, according to the reſult of the given Experiment.	
Firſt Set.	lbs.	hrs.	min.
In the Experiment No. 27. —	12.06	4	20
In the Experiment No. 28. —	12.70	4	20
Sum —	24.77	8	40
Means —	12.385	4	20
Second Set.			
In the Experiment No. 29. —	12.54	4	31
In the Experiment No. 30. —	12.28	2	59
Sum —	24.82	7	30
Means —	12.41	3	45

The mean reſults of theſe two ſets of Experiments differ very little from each other in appearance; and from this circumſtance I ſhall prove, that the new Boiler is better adapted for ſaving Fuel than the old.

By comparing the reſults of the Experiments made with the ſame Boiler, but with different quantities of water, we ſhall conſtantly find that the expence of Fuel was *leſs* in proportion as the quantity of water was *greater*. In the Experiment No. 23, when 127 lbs. of water were uſed, the reſult of the Experiment indicated that no more than 12.74 lbs. of ice-cold water could be made to boil with the heat generated in the combuſtion of 1 lb. of the Fuel uſed; but in the Experiment No. 26, made with the ſame Boiler, but when 4 times as much water was uſed, or 508 lbs., it appeared from the reſult of the Experiment, that 19.01 lbs. of ice-cold water might be made to boil with 1 lb. of the Fuel.

Now, in the firſt ſet of the Experiments we are comparing, as the quantity of water uſed (12,508 lbs.) was much greater than that uſed in the ſecond ſet (8120 lbs.), it is evident, that if the conſtruction of the machinery and the Management of the Fire had been equally perfect in the two caſes, the Economy of Fuel would have been greateſt where the largeſt quantity of water was uſed;—that is to ſay, in the firſt ſet of Experiments;—but, as that was not the caſe, it is certain that the Boiler uſed in the ſecond ſet is better adapted to economize Fuel than that uſed in the firſt.

But we need not go ſo far to ſearch for proofs of that fact. The reſult of the Experiment No. 31

is alone ſufficient to put the matter beyond doubt. In this Experiment, in which the quantity of water (though ſtill conſiderably ſhort of that uſed in the former ſet of Experiments) was augmented from 8120 lbs. to 11,368 lbs. the ſaving of Fuel was ſo much increaſed as to ſhow in a deciſive manner the ſuperiority of the new Boiler.

	Quantity of ice-cold water made to boil with 1lb. of the Fuel.	Time required to make ice-cold water boil, according to the reſult of the Experiment.	
The Preciſe Reſults	lbs.	hrs.	min.
Of this Experiment (No. 31.) were as follows, — — —	14.59	3	37
In the Experiments No. 27 and No. 28, they were, at a medium,	12.385	4	20

The difference in the expence of Fuel in theſe Experiments with theſe two Boilers is by no means inconſiderable; it amounts to above 14 *per cent.* and would have amounted to more, if more time had been allowed for heating the water in the Experiment with the new Boiler; for it is eaſy to ſhow—(what indeed was clearly indicated by all the Experiments)—that, in cauſing liquids to boil, the quantity of Fuel will be leſs, in proportion as the time employed in that proceſs is long; or, which is the ſame,—as the Fire is ſmaller: And the ſaving of Fuel ariſing from any given prolongation of the proceſs, will be the greater as the fire-place is more perfect, and as the means uſed for confining the heat are more effectual.

Though the general reſults of theſe two ſets of Experiments afforded abundant reaſon to conclude that the alterations I had introduced in arranging the new Boiler were real improvements; yet, when I compared the quantity of Fuel conſumed in the Experiments with this new Boiler, with the much ſmaller quantities, in proportion to the quantity of water, which were employed in ſome of my former Experiments with kitchen Boilers, I was for ſome time quite at a loſs to account for this difference. In all my Experiments with Boilers of different ſizes, from the ſmalleſt ſaucepan up to the largeſt kitchen Boilers, I had invariably found that the *larger* the quantity of water was, which was heated, the *leſs*, in proportion, was the quantity of Fuel neceſſary to be employed in that proceſs; and ſo entirely had that prejudice taken poſſeſſion of my mind, that when the ſtrongeſt reaſons for doubt preſented themſelves, they were overlooked; and it was not till I had ſearched in vain on every ſide to diſcover ſome other cauſe to which I could attribute the unexpected appearance that embarraſſed me, that I was induced,—I may ſay forced,—to abandon my former opinion, and to be convinced that what I had too haſtily conſidered as a general law, does not in fact obtain but within narrow limits;—that although, in heating *certain quantities* of liquids, there is an advantage in point of the Economy of Fuel in performing the proceſs on a larger ſcale, in preference to a ſmaller one; yet, when the liquid to be heated amounts to a certain quantity, this advantage ceaſes; and if it exceeds that quantity, it is attended with an expence of Fuel proportionally greater than when the quantity is leſs.

What the ſize of a Boiler muſt be, in order that the ſaving of Fuel may be a *maximum*, I do not pretend to have determined; I think however that there are ſome reaſons for ſuſpecting that it would not be larger than ſome of the kitchen Boilers uſed in my Experiments. But I recollect to have promiſed my Reader, that I would not give him my opinions, without laying before him at the ſame time the grounds of thoſe opinions.—In the preſent caſe they are as follows:

In an Experiment of which I have already given an account (No. 3.), $7\frac{15}{16}$lbs. of water, at the temperature of 58°, were made to boil in a ſaucepan fitted up in my beſt manner, in a cloſed fire-place; and the wood conſumed was 1 lb. This gives for the *preciſe reſult* of the Experiment, 6.68 lbs. of ice-cold water made to boil with 1 lb. of the Fuel.

In another Experiment, (No. 12.) made with one of the ſmall Boilers belonging to the kitchen of the Military Academy, fitted up on the ſame principles, 43.63 lbs. of water, at the temperature of 60°, were made to boil with 3 lbs. of wood. This gives 11.93 lbs. of ice-cold water made to boil with 1 lb. of the Fuel.

Again, in the Experiment No. 20, which was made with a larger Boiler, belonging to the ſame kitchen, and fitted up in the ſame manner, 187 lbs. of water, (equal to about 28 gallons,) at the temperature of 55°, were made to boil with the combuſtion of 8 lbs. of fire-wood. This gives 20.10 lbs. of ice-cold water made to boil with 1 lb. of the wood;—and farther than this I have not been able to puſh the Economy of Fuel.

In the Experiment No. 26. a Boiler was uſed, which had been conſtructed with the expreſs view to ſee how far it was poſſible to carry the Economy of Fuel in culinary proceſſes; and it was fitted up with the utmoſt care, and on the moſt approved principles. As I thought at that time that a large-ſized Boiler was eſſential to the economizing of Fuel, this Boiler was made to contain 106 gallons. In the Experiment in queſtion it actually contained 508 Bavarian pounds of water, (or about 63 gallons,) at the temperature of 48°; and to make this water boil, 24 lbs. of wood were conſumed. This gives 19.01 lbs. of ice-cold water made to boil with 1 lb. of Fuel. Hence it appears that the expence of Fuel was greater in this Experiment than in that laſt-mentioned.

Again, in the Experiment No. 31. when no leſs than 11,368 lbs. or 1685 gallons, of water were heated and made to boil in the new Brewhouſe Boiler; the wood conſumed amounted to 650 lbs. which (as the temperature of the water at the beginning of the Experiment was 65½°) gives for the *preciſe reſult* of the Experiment, 14.59 lbs. of ice-cold water made to boil with the heat generated in the combuſtion of 1 lb. of the Fuel.

As the relative quantities of Fuel expended in the Experiments are inverſely as the numbers expreſſing the quantities of ice-cold water, which, from the reſult of each Experiment, it appears might have been heated 180 degrees, or made to boil, under the mean preſſure of the atmoſphere at the level of the ſea, with the heat generated in the combuſtion of 1 lb. of the Fuel; it is evident that theſe numbers meaſure very accurately the different degrees to which the Economy of Fuel was carried in the different Experiments. The Economy of Fuel in heating liquids *depending on the quantity of the liquid*, as ſhown by the foregoing Experiments, may therefore be expreſſed ſhortly in the following manner:

	Quantity of water heated in the Experiment, *in Bavarian* lbs.	Degrees to which the Economy of the Fuel was carried.
	lbs.	lbs.
In the Experiment No. 3,	7·93	6.68
No. 12,	43·63	11.93
No. 16,	187	20.10
No. 26,	508	19.01
No. 31,	11,368	14.59

Before I take my leave of this ſubject I would juſt remark, that the cauſe of the appearances obſerved in the Experiments may, I think, be traced to that property of flame from which it has been denominated a non-conductor of heat:—For if the different particles of flame give off their heat only to bodies with which they actually come into contact, the quantity of heat given off by it will be,—*not as its volume*, (and conſequently not as the quantity of Fuel conſumed,) but rather *as its ſurface*. And as the ſurface of the flame, when fire-places are ſimilar, is proportionally greater in ſmall than in large fire-places;—the ſurfaces of ſimilar bodies being as the *ſquares* of their correſponding ſides, while their volumes are as the *cubes* of thoſe ſides;—it is evident that, on that account, leſs heat in proportion to the quantity generated in the combuſtion of the Fuel ought to be communicated to the Boiler, when the fire-place and Boiler are large, than when the proceſs is carried on upon a ſmaller ſcale.

There are, however, ſeveral other circumſtances to be taken into the account in determining the effects *of ſize* in the machinery neceſſary for boiling liquids; and one of them, which has great influence, is the heat abſorbed by the maſonry of the fire-place. This loſs will moſt undoubtedly be the ſmaller, as the fire-place is larger; but to determine the exact point when, the ſaving on the one hand being juſt counterbalanced by the loſs on the other, any augmentation or diminution of ſize in the machinery would be attended with a poſitive loſs of heat, is not eaſy to be aſcertained. Provided however that proper attention be paid to the *Management of the Fire*, and that as much heat as poſſible be generated in the combuſtion of the Fuel —(which may always be done in the largeſt fire-place as well, if not better, than in ſmaller ones); as that part of the heat which goes off in the ſmoke is indubitably loſt, a thermometer placed in the chimney would indicate, with a conſiderable degree of preciſion, the perfections or imperfections of the fire-place.

It is well known that the ſmoke which riſes from the chimnies of the cloſed fire-places of very large Boilers is much hotter than that which eſcapes from ſmaller fire-places; and I am ſurpriſed that this fact, which has long been known to me, ſhould not have led me to ſuſpect that the waſte of Fuel was proportionally greater in theſe large fire-places than in ſmaller ones.

Beſides the Experiments of which I have given an account, ſeveral others were made with the new Brewhouſe Boiler; and, among others, four Experiments were made on four ſucceeding days, in brewing Beer; and it was found that conſiderably leſs Fuel was expended in theſe trials, than was neceſſary in brewing the ſame quantity of beer in the other Brewhouſe, in which I firſt introduced my improvements. But though the alteration of form, diminution of the thickneſs of the metal, &c. which I had introduced in conſtructing the new Boiler, and alſo in the manner of fitting it up, had produced a conſiderable ſaving of Fuel, yet it was not accompanied by a proportional ſaving of time. I had flattered myſelf that by making the Boiler *very thin* and *very ſhallow*, I ſhould bring its contents to boil in *a very ſhort time;* but I did not conſider how much time is neceſſary for the combuſtion of the Fuel *neceſſary* for heating ſo large a quantity of water; otherwiſe my expectations on this head would have been leſs ſanguine. The quantity of heat generated in any given time being as the quantity of Fuel conſumed, it muſt depend in a great meaſure on the ſize of the fire-place; and when it is required to heat a large quantity of water, or of any liquid, in a very ſhort time, either the fire-place muſt be large, or (what in my opinion would be ſtill better) a number of ſeparate fire-places,—two or three, for inſtance, or even a greater number,—may be made under the ſame Boiler. The Boiler ſhould be made wide and ſhallow, in order to admit of a great number of flues, in which the flame and ſmoke of the different Fires ſhould be made to circulate ſeparately *under its bottom*.

The combuſtion of the Fuel, and conſequently the generation and communication of the heat, may in the ſame fire-place be conſiderably accelerated by increaſing the draught (as it is called) of the Fire; which may be done by increaſing the height of the chimney, or by enlarging the canal leading to the chimney, and keeping the damper open, when that paſſage is too ſmall;—or by ſhortening the length of the flues.

The maſter brewer having expreſſed a wiſh that ſome contrivance might be uſed by which the water might be made to boil a little ſooner in the new Boiler, I made an alteration in its fire-place which completely anſwered that purpoſe.

But beſides the deſire I had to oblige the maſter brewer, (who only thought how he could contrive to finiſh as early as poſſible his day's work,) I had another, and much more important object in view. Having had reaſon to ſuſpect that flues which go round on the outſide of large Boilers do little more than prevent the eſcape of the heat by their ſides,—which, with infinitely leſs trouble and leſs expence, may be prevented by other means,—I was deſirous of finding out, by a deciſive Experiment, the real amount of the advantages gained by thoſe flues; or the ſaving of Fuel which they produce. And as I was confident that the ſuppreſſion of the flue which went round the new Boiler would increaſe the draught of the fire-place, and accelerate the combuſtion of the Fuel, I concluded that if my opinion was well founded with reſpect to the ſmallneſs of the advantages derived from theſe *ſide flues*, the increaſe of heat ariſing from the acceleration of the combuſtion occaſioned by the increaſed draught on cloſing them up would more than counterbalance the loſs of thoſe advantages, and the time employed in heating the water would be found to be actually leſs than it was before.

The reſults of the following Experiments ſhow how far my ſuſpicions were founded.

Experiment, No. 32.

The flue round the outſide of the new Brewhouſe Boiler having been cloſed up, and two canals (*a* and *b*, Fig. 21.) formed from the end of the two outſide flues of thoſe ſituated *under* the Boiler, by which two canals (which were both furniſhed with dampers) the ſmoke paſſed off from under the Boiler directly into the chimney; the Experiment No. 31. which was made with the ſame Boiler before the outſide flues were cloſed up, was now repeated with the utmoſt care, in order to aſcertain the effects which the cloſing up of thoſe flues would produce. The quantity of water in the Boiler, and its temperature at the beginning of the Experiment, were the ſame;—the wood uſed as Fuel was taken from the ſame parcel, and it was put into the fire-place in the *ſame quantities*, and at the *ſame intervals of time;*—in ſhort, every circumſtance was the ſame in the two Experiments, excepting only the alterations which had been made in the fire-place. As the length of the flues through which the flame and ſmoke were obliged to paſs to get into the chimney had been diminiſhed more than half, (or reduced from 70 to above 30 feet,) the ſtrength of the draught of the fire-place was much increaſed, as was evident not only from the increaſed violence of the combuſtion of the Fuel, which was very apparent, but alſo from another circumſtance, which I think it my duty to mention. Before the flue round the Boiler was cloſed, if too much Fuel was put into the fire-place at once, it not only did not burn with a clear flame, but frequently the ſmoke, and ſometimes the flame, came out of the fire-place door, even when the damper in the chimney was wide open; but after this flue was cloſed up, it was found to be hardly poſſible to overcharge the fire-place, and the Fuel always burned with the utmoſt vivacity.

I ought to inform my Reader, that though the entrance into the flue which went round the outſide of the Boiler was cloſed, and another and a ſhorter road opened for the flame and ſmoke to paſs off into the chimney, yet the *cavity* of the flue remained; and by means of openings (c c c c c c Fig. 21. Plate V.) about ſix inches ſquare in the brick-work which ſeparated this old road (which was now ſhut up) from the flues *under the Boiler*, the flame was permitted to paſs into this cavity, and to ſpread itſelf round the outſide of the Boiler. This contrivance (which I would recommend for all Boilers) not only prevents the eſcape of the heat out of the Boiler by its ſides, but contributes ſomething towards heating it; and as the openings in the ſides of the flues do not ſenſibly impede the motion of the flame, they can do no harm.

As the two Experiments, the reſults of which I am about to compare, were made with the greateſt care, and as they are on ſeveral accounts uncommonly intereſting, I ſhall place them in a conſpicuous point of view.

A COMPARATIVE VIEW of TWO EXPERIMENTS made with a new Brewhouſe Boiler.

The time is reckoned from the beginning of the Experiment, and was the ſame in both Experiments.

Quantity of water in the Boiler 11,368 *lbs. Bavarian weight.*

Time from the beginning of the Experiment.		Fuel put into the fire-place.		Heat of the water in the Boiler.	
		Number of billets.	Quantity in weight.	Experiment No. 31, (outſide flue open.)	Experiment No. 32, (outſide flue cloſed.)
hrs.	min.	No.	lbs.	degrees.	degrees.
—	—	80	100	65½	65½
0	16	40	50	79	82
0	30	40	50	90	94
0	53	40	50	107	110
1	6	40	50	118	122
1	23	40	50	130	135
1	37	40	50	140	147
1	54	40	50	155	160
2	9	40	50	165	171
2	24	40	50	175	182
2	39	40	50	182	191
2	54	40	50	200	—
2	59	—	—	—	boiled
3	7	—	—	boiled	—

Having found by comparing the reſults of theſe two Experiments, that I had loſt nothing in reſpect to the Economy of Fuel, by ſhutting up the outſide flue of my Boiler, I was now deſirous of aſcertaining how much I had gained in point of time, or how much the increaſed draught of the fire-place, in conſequence of its flues being ſhortened, enabled me to abridge the time employed in cauſing the contents of the Boiler to boil, in caſes in which it ſhould be advantageous to expedite that proceſs at the expence of a ſmall additional quantity of Fuel.

By the following Experiment, in which the combuſtion of the Fuel was made as rapid as poſſible by keeping the fire-place full of wood, and the regiſter in the aſh-pit door and the damper in the chimney conſtantly quite open, may be ſeen how far I ſucceeded in the attainment of that object.

Experiment, No. 33.

The Boiler contained 11,368 lbs. Bavarian weight of water at the temperature of 47°. The Fuel uſed was pine-wood, moderately ſeaſoned, in billets three feet four inches long, and ſplit into ſmall pieces of about 1 lb. each, that it might burn the more rapidly.

This Experiment was made the 29th of November 1796, the barometer ſtanding at 26 inches 8.7 lines Paris meaſure, and Fahrenheit's thermometer at 33°.

Time.		Fuel put into the fire-place.	Temperature of the water in the copper.
hrs.	min.	lbs.	degrees.
2	0	100	47
—	14	100	58
—	34	100	88
—	51	100	100
3	9	100	123
—	25	100	144
—	39	100	151
4	0	100	—
—	10	—	200
—	17	—	boiled
Time employed - 2	17	Wood conſumed - 800	

In the Experiment No. 32, the ſame quantity of water at the temperature of 65½°, was made to boil in 2 hours 59 minutes, with the conſumption of 625 lbs. of the ſame kind of wood. Had the water in this Experiment been as cold as it was in the Experiment No. 33, (namely at the temperature of 47°,) inſtead of 625 lbs., 705 lbs. of the Fuel would have been neceſſary; and the proceſs, inſtead of laſting 2 hours and 59 minutes, would have laſted 3 hours and 22 minutes.

Hence we may conclude, that to abridge 1 hour and 5 minutes of 3 hours and 22 minutes in the proceſs of boiling 11,368 lbs. of water, this cannot be done at a leſs additional expence of Fuel than that of 95 lbs. of pine-wood;—or, to abridge the time *one-third*, there muſt be an additional expence of about *one-eighth* more Fuel.

In ſome caſes it will be moſt profitable to ſave time; in others to economiſe Fuel;—and it will always be deſirable to be able to do either, as circumſtances may render moſt expedient.

From a compariſon of the quantities of Fuel conſumed, and conſequently of heat generated, in the ſame time, with the quantities of heat actually communicated to the water, in the Experiments No. 32 and No. 33, during this time, an idea may be formed of the great quantity of heat that may remain in flame and ſmoke after they have paſſed many feet in flues under the thin bottom of a Boiler containing cold water; and this ſhows with how much difficulty theſe hot vapours part with their heat, and how important it is to be acquainted with that fact in order to take meaſures with certainty for economiſing Fuel.

I have been the more particular in my account of theſe Experiments with large Boilers, as I believe no Experiments of the kind on ſo large a ſcale have been yet made; and as they were all conducted with care, their reſults have intrinſic value independent of the particular uſes to which I have applied them.

Of the relative Quantities of Heat producible from different Kinds of Fuel.

As in the countries where this Eſſay is likely to be moſt read, pit-coals are more frequently uſed as Fuel than wood, it will not only be ſatisfactory, but in many caſes may be really uſeful to my Reader to know the relative quantities of heat producible from coals and from wood; in order to be able to compare the reſults of Experiments in which coals are uſed as Fuel, with thoſe of which I have here given an account; or to determine the quantity of coals neceſſary in any proceſs which it is known may be performed with a given quantity of wood.

It was my intention to have made a ſet of Experiments on purpoſe to determine the relative quantities of heat producible from all the various kinds of combuſtible bodies which are uſed as Fuel; and I made preparations for beginning them, but I have not yet been able to find leiſure to attend to the ſubject.

The moſt ſatisfactory account I have been able to procure reſpecting the matter in queſtion, is one for which I am indebted to my friend Mr. KIRWAN. By this account, which he tells me is founded on Experiments made by Mr. *Lavoiſier*, it appears, that equal quantities of water, under equal ſurfaces, may be evaporated, and conſequently equal heats produced,

By 403 lbs. of Coaks	or in meaſure	By 17 of Coaks
600 — of Pit-coal		10 of Pit-coal
600 — of Charcoal		40 of Charcoal
1089 — of Oak		33 of Oak.

I wiſh I were at liberty to tranſcribe the ingenious and intereſting obſervations that accompanied this eſtimate; but as they make part of a work which I underſtand is preparing for the Preſs, I dare not anticipate what Mr. KIRWAN will himſelf ſoon lay before the Public.

According to this eſtimate it appears that 1089 lbs. of oak produces as much heat in its combuſtion as 600 lbs. of pit-coal. Now, if we ſuppoſe that the pine-wood uſed in my Experiments is capable of producing as much heat *per pound* as oak,—and I have reaſon to think it does not afford leſs,—from the quantity of pine-wood uſed in any of my Experiments, it is eaſy to aſcertain how much coal would have been neceſſary to generate the ſame quantity of heat; for the weight of the coal which would be required is, to the weight of the wood actually conſumed, as 600 to 1089.

In one of my Experiments (No. 31,)—11,368 lbs. of water, at the temperature of 65½°, were made to boil with 650 lbs. of pine-wood. As, when the Experiment was made, the mercury in the barometer ſtood at about 28 Engliſh inches, the temperature of the water when it boiled was only 209½°, conſequently its temperature was raiſed (209½—65½) 144 degrees. Had the water been boiled in London, or in any other place nearly on a level with the ſurface of the ſea, it muſt have been heated to 212° to have been made to boil, conſequently its temperature muſt have been raiſed $146\frac{1}{2}$°; and to have done this, inſtead of 650 lbs. of wood, $661\frac{1}{2}$ lbs. would have been required; (140° is to 650 lbs. as $146\frac{1}{2}$° to $661\frac{1}{2}$ lbs.

If pit-coal were uſed inſtead of wood, $363\frac{1}{2}$ lbs. of that kind of Fuel would have been ſufficient;—for the quantities in weight of different kinds of Fuel required to perform the ſame proceſs being inverſely as the quantities of heat which equal weights of the given kinds of Fuel are capable of generating, or directly as the quantities of the kind of Fuel in queſtion, which are required to produce the ſame heat, it is 1089 to 600, as $661\frac{1}{2}$ lbs. of wood to $363\frac{1}{2}$ lbs. of coal, ſuppoſing the foregoing eſtimate to be exact.

Whether it would be poſſible to cauſe ſo large a quantity of water, (1681 wine gallons,) at the given temperature, ($65\frac{1}{2}$°,) to boil, with this ſmall quantity of coal, I leave to thoſe who are converſant in Experiments of this kind to determine.

From the reſult of my 20th Experiment it appeared that $20\frac{1}{10}$ lbs. of ice-cold water might be heated 180 degrees, or made to boil under the mean preſſure of the atmoſphere at the level of the ſurface of the ocean, with the heat generated in the combuſtion of 1 lb. of pine-wood. Computing from the reſult of this Experiment, and from the relative quantities of heat, producible from pine-wood, and from pit-coal, it appears that the heat generated in the combuſtion of 1 lb. of pit-coal, would make 36$\frac{5}{10}$ lbs. of ice-cold water boil.

Hence it appears that pit-coal ſhould heat 36 times its weight of water, from the freezing point to that of boiling;—and as it has been found by Experiments made with great care by Mr. WATT, that nearly 5$\frac{1}{4}$ times as much heat as is ſufficient to heat any given quantity of ice-cold water to the boiling point, is required to reduce the ſame quantity of water, *already boiling-hot*, to ſteam; according to this eſtimation, the heat generated in the combuſtion of 1 lb. of coal ſhould be ſufficient to reduce very nearly 7 lbs. of boiling-hot water to ſteam.

How far theſe eſtimates agree with the Experiments that have been made with ſteam-engines, I know not; but there ſeems to be much reaſon to ſuſpect that the expence of Fuel, in working thoſe engines, is conſiderably greater than it ought to be, or than it would be, were the Boilers and Fire-places conſtructed on the beſt principles, and the Fire properly managed.

An Eſtimate of the real Amount of the Loſs of Heat in culinary Proceſſes.

IN attempts to improve, it is always very deſirable to know exactly what progreſs has been made;—to be able to meaſure the diſtance we have laid behind us in our advances; and alſo that which ſtill remains between us and the object in view. The ground which has been gone over is eaſily meaſured; but to eſtimate that which ſtill lies before us is frequently much more difficult.

The advances I have made in my attempts to improve fire-places, for the purpoſe of economiſing Fuel, may be eſtimated by the reſults of the Experiments of which I have given an account in this Eſſay; but it would be ſatisfactory, no doubt, to know how much farther it is poſſible to puſh the Economy of Fuel.

In my 4th Experiment, $7\frac{15}{18}$ lbs. of water, at the temperature of 58°, were made to boil, at Munich, with 6 lbs. of wood. If, from the reſult of this Experiment, we compute the quantity of ice-cold water which, with the heat generated in the combuſtion of 1 lb. of the Fuel, might be heated 180 degrees, or made to boil, it will turn out to be only $1\frac{1}{7}$ lb. or more exactly 1.11 lb.

According to the reſult of the Experiment No. 20, it appeared, that no leſs than $20\frac{1}{10}$ lbs. of ice-cold water might have been made to boil with the heat generated in the combuſtion of 1 lb. of pine-wood.

It appears, therefore, that about *eighteen times* as much Fuel, in proportion to the quantity of water heated, was expended in the Experiment No. 4, as in that No. 20; and hence we may conclude with the utmoſt certainty, that of the heat generated, or which, with proper management, might have been generated in the combuſtion of the Fuel uſed in the 4th Experiment, leſs than $\frac{1}{18}$ part was employed in heating the water; — the remainder, amounting to more than $\frac{17}{18}$ of the whole quantity, being diſperſed and loſt.

I ventured to give it as my opinion in the beginning of this Eſſay, that "not leſs than *ſeven-eighths* of the heat generated, or which, with proper ma-" nagement, might be generated from the Fuel ac-" tually conſumed, is carried up into the atmoſphere " with the ſmoke, and totally loſt."—I will leave it to my Reader to judge whether this opinion was not founded on good and ſufficient grounds.

But though it be proved beyond the poſſibility of a doubt, that the proceſs of heating water was performed in the 20th Experiment with about $\frac{1}{18}$ part of the proportion of Fuel which was actually expended in the 4th Experiment, yet neither of theſe Experiments, nor any deductions that can be founded on their reſults, can give us any light with reſpect to the *real* loſs of heat, or how much leſs Fuel would be ſufficient were there no loſs whatever of heat. The Experiments ſhow that the loſs of heat muſt have been at leaſt *eighteen times* greater in one caſe than in the other; but they do not afford grounds to form even a probable conjecture reſpecting the amount of the loſs of heat in the Experiment in which the Economy of Fuel was carried the fartheſt, or the poſſibility of any farther improvements in the conſtruction of fireplaces. I ſhall, however, by availing myſelf of the labours of others, and comparing the reſults of their Experiments with mine, endeavour to throw ſome light on this abſtruſe ſubject.

Doctor Crawford found, by an Experiment contrived with much ingenuity, and which appears to have been executed with the utmoſt care, that the heat generated in the combuſtion of 30 grains of charcoal raiſed the temperature of 31 lbs. 7 oz. Troy=181,920 grains of water, $1\frac{71}{100}$ degrees of Fahrenheit's thermometer, *when none of the heat generated was ſuffered to eſcape.*

But if 30 grains of charcoal are neceſſary to raiſe the temperature of 181,920 grains of water $1\frac{71}{100}$ degrees, it would require 3157.9 grains of charcoal to raiſe the temperature of the ſame quantity of water 180 degrees, or from the point of freezing to that of boiling; for it is 1.71° to 30 grains, as 180° to 3157.9 grains. Conſequently the heat generated in the combuſtion of 1 lb. of charcoal would be ſufficient to heat 57.608 lbs. of ice-cold water 180 degrees, or to make it boil;—for 3157.9 grains of charcoal are to 181,920 grains of water as 1 lb. of charcoal to 57.608 lbs. of water.

From the reſults of Mr. Lavoisier's Experiments, it appeared that the quantities of heat generated in the combuſtion of equal weights of charcoal and dry oak, are as 1089 to 600. Hence we may conclude, that equal quantities of heat are generated by 1 lb. of charcoal and 1.815 lbs. of oak; conſequently that the heat generated in the combuſtion of 1.815 lbs. of oak, would heat 57.608 lbs. of ice-cold water,—or 1 lb. of oak, 31.74 lbs. of ice-cold water 180 degrees, or cauſe it to boil;—*were no part of the heat generated in the combuſtion of the Fuel loſt.*

If now we ſuppoſe the quantities of heat producible from equal weights of dry oak and of dry pine-wood to be equal,—(and there is reaſon to believe that this ſuppoſition cannot be far from the truth,)—we can eſtimate the *real loſs of heat* in each of the two Experiments before mentioned, (No. 4 and No. 20,) as alſo in every other caſe in which the quantity of Fuel conſumed, and the effects produced by the heat, are known.

Thus, for inſtance, in the 20th Experiment, as the effects actually indicated that with *that part* of the heat generated in the combuſtion of 1 lb. of the Fuel which *entered the Boiler*, $20\frac{1}{10}$ lbs. of ice-cold water might have been made to boil; as by the above eſtimate it appears that $31\frac{74}{100}$ lbs. of ice-cold water might be made to boil with *all* the heat generated in the combuſtion of 1 lb. of the Fuel—it is evident that about *one-third* of the heat generated, was loſt; or $\frac{20.1}{31.74}$ of it was ſaved.

This loſs is certainly not greater than might reaſonably have been expected, eſpecially when we conſider all the various cauſes which conſpire in producing it; and I doubt whether the Economy of Fuel will ever be carried much farther.

In the Experiment No. 4, as the effects produced by the heat which entered the Boiler indicated that no more than 1.14 lb. of ice-cold water could have been made to boil with 1 lb. of the Fuel, it appears that in this Experiment only about $\frac{1}{28}$th part of the heat generated was ſaved.

In all the Experiments made on a very large ſcale, with Brewhouſe Boilers, rather more than *one-half* of the heat generated found its way up the chimney, and was loſt.

CHAP. VI.

A ſhort Account of a Number of Kitchens, public and private, and Fire-places for various Uſes, which have been conſtructed under the Direction of the Author, in different Places.—Of the Kitchen of the HOUSE *of* INDUSTRY *at* MUNICH—*Of that of the* MILITARY ACADEMY—*Of that of the* MILITARY MESS-HOUSE—*that of the* FARM-HOUSE, *and thoſe belonging to the* INN *in the* ENGLISH GARDEN *at* MUNICH.—*Of the Kitchens of the Hoſpitals of* LA PIETA; *and* LA MISERICORDIA *at* VERONA.—*Of a ſmall Kitchen fitted up as a Model in the Houſe of* SIR JOHN SINCLAIR *Bart. in* LONDON.—*Of the Kitchen of the* FOUNDLING HOSPITAL *in* LONDON.—*Of a* MILITARY KITCHEN *for the Uſe of* TROOPS *in* CAMP.—*Of a* PORTABLE BOILER *for the Uſe of* TROOPS *on a* MARCH.—*Of a large* BOILER *fitted up as a Model for* BLEACHERS *at the* LINEN-HALL *in* DUBLIN.—*Of a Fire-place for* COOKING, *and at the ſame Time* WARMING A LARGE HALL; *and of a* PERPETUAL OVEN, *both fitted up in the* HOUSE *of* INDUSTRY *at* DUBLIN.—*Of the* KITCHEN—LAUNDRY—CHIMNEY FIRE-PLACES—COTTAGE FIRE-PLACE—*and Model*

of

of a LIME-KILN—*fitted up in* IRELAND, *in the Houſe of the* DUBLIN SOCIETY.

MY wiſh to give the moſt complete information poſſible, with regard to the *grounds* on which the improvements I propoſe are founded, has induced me to be very particular in my account of my Experiments, and of the concluſions and practical inferences I have thought myſelf authoriſed to draw from them; and as theſe inveſtigations have frequently led me into abſtruſe philoſophical diſquiſitions, which might not perhaps be very intereſting to many of my readers, to whom a ſimple account of my Fire-places, with directions for conſtructing them, might be really uſeful; in order to accommodate readers of all deſcriptions, I have thought it beſt to divide my ſubject, and to reſerve what I have ſtill to ſay on the *mechanical part* of it,—the Conſtruction of Kitchen Fire-places,—for a ſeparate Eſſay. In the mean time, for the information of thoſe who may have opportunities of examining any of the Kitchens,—or Fire-places, for other purpoſes,—which have already been conſtructed on my principles, under my direction, I have annexed the following account of them, and of the particular merits and imperfections of each of them. This account, added to what has been ſaid in the foregoing Chapters of this Eſſay on the conſtruction of Fire-places, will, I flatter myſelf, be found ſufficient to convey the fulleſt information reſpecting the ſubject under conſideration, and enable thoſe, who may wiſh to adopt the propoſed improvements, to conſtruct Fire-places of all kinds on the principles recommended, *without any farther aſſiſtance.*

Thoſe who may not have leiſure to enter into theſe ſcientific inveſtigations, and who, notwithſtanding, may wiſh to imitate theſe inventions, will find all the information they can want in my next Eſſay.

An Account of the Kitchen of the HOUSE *of* INDUSTRY *at* MUNICH, *in its preſent State.*

THE large circular copper boiler (which is ſituated in a ſmall room adjoining to the great Kitchen) is fitted up in a very complete manner;—its (wooden) cover is cheap, ſimple, and durable, and anſwers perfectly well for confining the heat;—the ſteam tube (or ſteam chimney, as I have called it) is very uſeful, as it carries off all the ſteam generated in cooking, and keeps the air of the Kitchen dry and wholeſome.—To carry off the ſteam which riſes from the hot ſoup when it is ſerved up, there is a ſteam-chimney of wood, (furniſhed with a valve,) the opening of which is ſituated at the higheſt part of the Kitchen. To prevent the cold air from coming down by this paſſage into the Kitchen, its damper (which is opened and ſhut by a cord which goes over a pulley) is, in winter, kept conſtantly ſhut, except juſt when it is neceſſary to open it for a moment to let out the ſteam.

The only alteration I would make, were I to fit up this boiler again, would be to leave openings by which the flues might be cleaned occaſionally, without lifting the boiler out of its place.—This ſhould be done in the fire-places of all large boilers. This boiler, which is uſed every day, requires to have its flues cleaned, and its bottom and ſides ſcrubbed with a broom, to free them of ſoot, once in ſix weeks.

Over againſt this boiler is a machine for drying potatoes, which has been found to anſwer perfectly well the end for which it was contrived. Potatoes firſt moderately boiled, and then ſkinned and cut into thin ſlices, and dried in this machine, may be kept good for many years.

The eight iron boilers in the *great Kitchen* are fitted up on good principles; and the oven which is heated by the ſmoke from the fire-places of two of theſe boilers, (which oven is deſtined for drying the wood for the uſe of this Kitchen,) is deſerving of attention.

The wooden covers of theſe eight boilers, and the horizontal tubes, conſtructed of wood wound round with canvas and painted with oil colours, by which the ſteam is carried off, have been found to anſwer very well the purpoſes for which they were contrived.

The Kitchen of the Military Academy at MUNICH.

This Kitchen in its preſent ſtate is ſo perfect in all its parts, that I do not think it capable of any conſiderable improvement. The *roaſter*, which has been in daily uſe *ſeven years*, is ſtill in good condition, and bids fair to laſt *twenty years* longer.—It is large and roomy, and has been found to be extremely uſeful. Though the different parts of this Kitchen are not diſtributed with ſo much ſymmetry as could have been wiſhed, owing to local circumſtances, yet it is very complete in its various details, and all the various proceſſes of cookery are performed in it with little labour, and with a very ſmall expence indeed of Fuel. Two large boilers and three large ſtewpans, which are fitted up in a detached maſs of brick-work in a corner of the room, (on the right hand on going into it,) I can recommend as perfect models for imitation. In ſhort, I know of nothing that I could wiſh to alter in this Kitchen.—To ſay the truth, it has already undergone a ſufficient number of changes and alterations.

The Kitchen in the Military Hall or Officers' Meſs-Houſe in the Engliſh Garden at MUNICH.

This Kitchen is much leſs perfect in its details than that juſt mentioned.—It was built in the ſpring of the year 1790, and has ſince undergone only a few trifling alterations. It has three roaſters, which are made ſmall on purpoſe to ſerve as models for private families; and I have had the pleaſure to know that they have often been imitated.

The Kitchen in the Farm Houſe in the Engliſh Garden.

This Kitchen is well contrived for the uſe for which it was deſigned, and I can recommend it as a very good model for the Kitchens of Farm-houſes, for families conſiſting of eighteen or twenty perſons. One of the boilers, which is deſtined for warming water for the uſe of the Kitchen and the ſtables, is, in winter, heated by the ſmoke of a German ſtove, which is ſituated in an adjoining room,—that inhabited by the Overſeer of the farm.

The great Kitchen of the Inn in the Garden.

This Kitchen, which is adjoining to the Farm-houſe, is contrived almoſt for the ſole purpoſe of roaſting chickens before an open fire, a kind of food of which the Bavarians are extravagantly fond. It has three open fire-places, conſtructed on the principles recommended in my Eſſay on Chimney Fire-places, fronting different ſides of the Kitchen, and all opening into the ſame chimney, which chimney is built nearly in the middle of the room. This Kitchen was built before my roaſters were come into uſe.

The ſmall Kitchen belonging to the Inn.

This Kitchen has nothing belonging to it that deſerves attention, or that I would recommend for imitation. It was originally deſigned merely for making coffee, chocolate, &c.

A Kitchen that has lately been fitted up on my principles, in the new Hoſpital for the infirm and helpleſs Poor, which is ſituated on the height called the *Gaſteig*, on the ſide of the river oppoſite to the town of Munich, is much more intereſting, and is a good model for imitation.

The Kitchen of the Hoſpital of La Piéta at VERONA

Is peculiarly intereſting, on account of its convenient form and the perfect ſymmetry of its parts.

The maſs of brick-work in which the boilers are fixed occupies the middle of one ſide of a large high room, which is plaſtered and white-waſhed, and neatly paved. The covers of the large boilers are lifted up by ropes which go over pullies fixed to the ceiling of the top of the room; but were I to build the Kitchen again, I ſhould ſubſtitute wooden covers with ſteam chimnies inſtead of them, ſuch in all reſpects as that belonging to the large round copper boiler in the Kitchen of the Houſe of Induſtry at Munich. When the covers are ſo large that they cannot conveniently be lifted on and off with the hand, they ſhould, in my opinion, always be made of wood, and divided into parts, united by hinges. When they are deſigned for confining the ſteam *entirely*, they ſhould be made on a peculiar conſtruction, which will hereafter be deſcribed. The covers for ſmall boilers, and thoſe for ſaucepans, ſhould always be of tin, and double.

The grates on which the fires are made under the boilers in the Kitchen of the Hoſpital of *La Piéta*, are circular; but they are not hollow, or diſhing, as that improvement did not occur to me till after that Kitchen was finiſhed. The ſpiral flues under the boilers are alſo wanting, and for the ſame reaſon. In all other reſpects this Kitchen is, I believe, quite perfect.

The Kitchen of the Hoſpital of La Miſericordia at VERONA

Is conſtructed on the ſame principles as that of *La Piéta*. The only difference between them is in the diſtribution of the boilers. That of *La Miſericordia* is built round two ſides of the room. In many caſes, this manner of diſpoſing of the boilers will be found more convenient than any other; but in all caſes where this method of placing them is preferred, care muſt be taken to place the largeſt boilers fartheſt from the chimney, and the ſmaller ones nearer to it, and in regular ſucceſſion as their ſizes diminiſh. This is neceſſary, in order that in the maſs of brick-work in which the boilers are fixed, there may be room behind the ſmaller boilers for the canals which carry off the ſmoke from the large ones into the chimney.

This circumſtance was attended to in conſtructing the ſmall Kitchen that I fitted up laſt ſpring in the houſe of Sir John Sinclair, Bart. Preſident of the Board of Agriculture, Whitehall, London. This Kitchen (which was intended to ſerve as a model, and is open to the public view at all hours) is by no means as perfect as I wiſhed it to be:— Having been built during my journey to Ireland, ſeveral miſtakes were made by the workmen I employed, who, though they have great merit in their different lines of buſineſs, had not *then* had ſufficient experience in conſtructing Kitchens on my principles, to be able to execute ſuch a job in my abſence without committing ſome faults. Thoſe which were moſt eſſential I corrected; but my ſtay in England after my return from Ireland was too ſhort, and my time too much taken up with other matters to rebuild the Kitchen from the foundation; which I was very deſirous of doing, and which, with the permiſſion of the Proprietor, I ſhall certainly do when I come to England again. The greateſt fault of the Kitchen is the want of dampers to the canals by which the ſmoke is carried off from the cloſed fire-places of the boilers and ſaucepans into the chimney. Theſe dampers ſhould never be omitted in any fire-place, however ſmall. They are neceſſary, even in fire-places for the ſmalleſt ſaucepans, and no large boiler ſhould on any account be without one. Some Experiments I have lately made (ſince my return to Bavaria) have ſhewed me how very neceſſary theſe dampers are; and I conſider it as my duty to the Public to loſe no time in recommending the general uſe of them. The flattering attention that has been paid by the Public to the various improvements I have taken the liberty to propoſe, not only demands my warmeſt gratitude, but lays me under an indiſpenſable obligation to exert myſelf to the utmoſt to deſerve their eſteem, and to merit the diſtinguiſhed marks of their confidence with which on ſo many occaſions I have been honoured.

But to return to the Kitchen in the houſe of Sir John Sinclair—(the place where the meetings of the board of Agriculture are held, and where of courſe there is a great concourſe of ingenious men from all parts of the kingdom,—of men zealous for the progreſs of uſeful improvements); as the room is very ſmall, it was not poſſible to do more in it than juſt to fit up a few ſmall boilers and ſaucepans; and one middling-ſized roaſter, ſuch as might ſerve for a ſmall family; which laſt is a machine ſo very uſeful that I cannot help flattering myſelf that it will ſoon come into general uſe. The ſaving of Fuel which it occaſions is almoſt incredible, and the meat roaſted in it is remarkably well-taſted and high-flavoured.

One of theſe roaſters, on a large ſcale, was put up, under my direction, in the Kitchen of the Foundling Hoſpital in London; and though I could not ſtay in England to ſee it finiſhed, I have had the ſatisfaction to learn, ſince my arrival at Munich, from my friend Mr. Bernard, (who is Treaſurer to the Hoſpital,) that it has anſwered even beyond his expectations. He informs me, that when 112 lbs. of beef are roaſted in it at once, the expence for Fuel amounts to no more than *four pence* ſterling; and this when the coals are reckoned at an uncommonly high price, namely, at 1 s. 4d. the buſhel.

In the roaſter belonging to the Kitchen of the Military Academy at Munich, I cauſed 100 lbs. Bavarian weight—(equal to 123.84 lbs. Avoirdupois)—of veal, in *ſix large pieces*, to be roaſted at once, as an Experiment; the Fuel conſumed was 33 lbs. Bavarian weight of dry pine-wood, (equal to 40.86 lbs. Avoirdupois,) which, (at 4½ florins the *claſter*, weighing 2967 lbs. Bavarian weight) coſt 3 creutzers, or about *one penny* ſterling.

This Experiment was made in the year 1792. Happening to mention the reſult of it in a large company in London, ſoon after my arrival there in the autumn of the year 1795, I had the mortification to perceive very plainly by the countenances of my hearers how dangerous it is to promulgate very extraordinary truths. I afterwards grew more cautious, and ſhould not now have ventured to publiſh this account, had not the reſults of Experiments equally ſurpriſing, which have been made with the roaſter in the Kitchen of the Foundling Hoſpital, been made known to the Public.

Not only the roaſter, but the boilers alſo which have been put up under my direction in the Kitchen of the FOUNDLING HOSPITAL, have been found to anſwer very well; and I am informed that ſeveral other great Hoſpitals are about to imitate them. As I left London before the Kitchen of the Foundling Hoſpital was entirely finiſhed, I do not know whether there are dampers to the canals by which the ſmoke goes off from the fire-places of the boilers, and from that of the roaſter, to the chimney. If there are not, I could wiſh they might ſtill be added; and I would ſtrongly recommend it to thoſe who may be engaged in conſtructing Kitchen fire-places on my principles, *never to omit them.*

Oval grates of caſt iron, in the form of a diſh, ſuch as I have deſcribed in the foregoing Chapters of this Eſſay, were tried in the Kitchen of the Foundling Hoſpital; but the heat was found to be ſo intenſe that they were ſoon melted and deſtroyed; and we were obliged to have recourſe to common flat grates, compoſed of ſtrong bars of caſt iron. Perhaps the heat generated in the combuſtion of pit-coal is ſo intenſe, when completely confined, (as it ought always to be in cloſed fire-places,) that it will not be poſſible, where coals are uſed as Fuel, to uſe the hollow diſhing grates I have introduced in the public kitchens at Munich, and which have been deſcribed and recommended in this Eſſay.

Since my return to Bavaria, I have made ſeveral Experiments with grates compoſed of common bricks, placed edgewiſe, and I find that they anſwer for that uſe full as well, if not better, than iron bars. By making bricks *on purpoſe* for this uſe, of proper forms and dimenſions, and compoſed of the beſt clay mixed with broken crucibles beaten to a coarſe powder, Kitchen fire-places might be fitted up with them, which would be both cheap and durable, and as perfect in all other reſpects as any that could poſſibly be made, even were the moſt coſtly materials to be uſed in their conſtruction.

To diminiſh ſtill farther the expence attending the conſtruction of cloſed Kitchen fire-places deſigned for the uſe of poor families, the opening by which Fuel is introduced might be cloſed with a brick, or with a flat ſtone;—another brick, or ſtone, might be made to ſerve at the ſame time as a regiſter and a door to the aſh-pit, and a third as a damper to the chimney or canal for carrying off the ſmoke from the fire-place.

I lately had an opportunity of fitting up a Kitchen on theſe principles, in the conſtruction of which *there was not a particle of iron uſed*, or of any other metal, except for the boiler. On the approach of the French army under General Moreau in Auguſt laſt, (1796,) the Bavarian troops being aſſembled at Munich, (under my command,) for the defence of the capital, the town was ſo full of ſoldiers that ſeveral regiments were obliged to be quartered in public buildings, and encamped on the ramparts, where they had no conveniencies for cooking. For the accommodation of a part of them, four large oblong ſquare boilers, compoſed of very thin ſheet copper well tinned, were fitted up in a maſs of brick-work in the form of a croſs; each boiler with its ſeparate fire-place, communicating by double canals, furniſhed with dampers, with one common chimney, which ſtands in the centre of the croſs. The dampers are thin flat tiles; the grates on which the Fuel is burned are compoſed of common bricks, placed edgewiſe;—and the paſſages leading to the fire-place, and to the aſh-pit, are cloſed by bricks which are made to ſlide in grooves.

Under the bottom of each boiler, which is quite flat, there are three flues, in the direction of its length; that in the middle, which is as wide as both the others, being occupied by the burning Fuel. The opening by which the Fuel is introduced is at the end of the boiler *fartheſt from the chimney;* and the flame running along the middle flue to the end of it, divides there, and returning in the two ſide flues to the hither end of the boiler, there riſes up into two other flues, in which it paſſes along the outſide of the boiler into the chimney. The boilers are furniſhed with wooden covers divided into two equal parts, united by hinges. In order that the four boilers may be tranſported with greater facility from place to place, (from one camp to another,

other, for inſtance,) they are not all preciſely of the ſame ſize, but one is ſo much leſs than the other, that they may be packed one in the other. The largeſt of them, which contains the three others, is packed in a wooden cheſt, which is made juſt large enough to receive it. In the ſmalleſt may be packed a circular tent, ſufficiently large to cover them all. In the middle of the tent there muſt be a hole through which the chimney muſt paſs. The four boilers, together with the tent, and all the apparatus and utenſils neceſſary for a Kitchen on this conſtruction for a regiment conſiſting of 1000 men, might eaſily be tranſported from place to place on an Iriſh car drawn by a ſingle horſe.

I have been the more particular in my account of this portable Kitchen, as I think it would be found very uſeful for troops in camp. The Right Honourable Mr. Thomas Pelham made a trial of one of them laſt ſummer for his regiment, (the Suſſex militia,) and found it to be very uſeful. The ſaving of Fuel was very conſiderable indeed; and the ſaving of trouble in cooking not leſs important. The firſt Experiment we made together in a ſingle boiler, fitted up for the purpoſe in the open air, in the middle of the court-yard of Lord Pelham's houſe in London.

I ought perhaps to have reſerved what I have here ſaid on the ſubject of theſe Military portable Kitchens for my Eſſay on Kitchen fire-places, where it would more naturally have found its place; but being perſuaded of the great advantages that may be derived from them, I am unwilling to loſe a moment in recommending them to the attention of thoſe who have it in their power to bring them into uſe.

Thoſe who wiſh to know more about them may, I am confident, procure every information they can deſire reſpecting them, by applying to Mr. Pelham, or to any of the Officers of the Suſſex militia who were in camp with the regiment laſt ſummer.

There is one more invention for the uſe of armies in the field, which I wiſh to recommend, and that is a *portable boiler* of a light and cheap conſtruction, in which victuals may be cooked *on a march.* There are ſo many occaſions when it would be very deſireable to be able to give ſoldiers, haraſſed and fatigued with ſevere ſervice, a warm meal, when it is impoſſible to ſtop to light fires and boil the pot, that I cannot help flattering myſelf that a contrivance, by which the pot *actually boiling* may be made to keep pace with the troops as they advance, will be an acceptable preſent to every humane officer, and wiſe and prudent general. Many a battle has undoubtedly been loſt for the want of a good comfortable meal of warm victuals to recruit the ſtrength and raiſe the ſpirits of troops fainting with hunger and exceſſive fatigue.

But to return from this digreſſion.—The form of the two principal boilers in the Kitchen of the Foundling Hospital is that of an oblong ſquare; that form, which, on ſeveral accounts, I have reaſon to think preferable to all others for large boilers, but eſpecially on account of the facility of fitting them up with ſquare bricks, and of cleaning their flues, I firſt introduced in Ireland in ſeveral fire-places deſigned for different uſes, which I fitted up as models, in Dublin, during the viſit I made laſt ſpring to that country on the invitation of my friend Mr. Secretary Pelham.

The firſt of theſe oblong ſquare boilers is that which is fitted up in the court-yard of the Linen-hall at Dublin, *as a model for bleachers:* it is 8 feet wide, 10 feet long, and 2 feet deep; and it is furniſhed with a wooden cover, which, ſhutting down in a groove in which there is a ſmall quantity of water, the ſteam is by theſe means confined in the boiler. This cover is moveable on its hinges, which are placed at the end of the boiler fartheſt from the door of the fire-place; and it is occaſionally lifted up by means of a rope that goes over a compound pully which is fixed over the boiler at the top or ceiling of the room.

Under this boiler there are five flues, which run in the direction of its length, and are arranged and conſtructed in the ſame manner as the flues of the new Brewhouſe boiler that I lately fitted up at Munich.—(See Fig. 21, Plate V.)—There are no flues round the outſide of this boiler; but the brick walls, by which they are defended from the cold air, are double, and the ſpace between them is filled with charcoal duſt.

The Fuel burns at the hither end of the middle flue, in an oval diſh-grate; and the flame running along in this flue under the middle of the boiler to the farther end of it, there divides, and returns in the two adjoining flues:—It then turns to the right and left, and going back again in the two outſide flues to the farther end of the boiler, goes out from under it, there, in two canals, which ſloping upwards conduct it to the flues of a *ſecond boiler* of equal dimenſions with the firſt, where it circulates, and warms the water which is deſigned for refilling the firſt boiler.

As theſe boilers are made of exceedingly thin ſheet-copper,—and *thin boilers* are ſtronger to reſiſt the effects of the Fire, and conſequently more durable, than very thick ones,—they both together coſt much leſs than one ſingle boiler on the common conſtruction; and Mr. Duffin, Secretary to the Linen-board, who is a very active, intelligent man, and is himſelf engaged in a large concern in the bleaching buſineſs, ſhowed me a computation founded on actual Experiments which he himſelf made with this new boiler, by which he proved that the ſaving of Fuel which will reſult from the general introduction of theſe boilers in the bleaching trade throughout Ireland will amount to at leaſt *fifty thouſand pounds ſterling a-year.*

In a LAUNDRY that I fitted up in the houſe belonging to the Dublin Society, (and which is deſigned to ſerve as a model for laundries for private gentlemen's families,) there are alſo two oblong ſquare boilers, the one heated by the Fire, and the other by the ſmoke; and this ſmoke, after having circulated in the flues under the ſecond boiler, paſſes through a long flue, (conſtructed like hot-houſe flues,) which goes round two ſides of the *drying-room*, (which is adjoining to the *waſhing-room*,) and then paſſing through the wall of the drying-room into the ironing-room, it goes off into an open chimney.

As the bottom of the ſecond boiler lies on a level with the top of the firſt, the warm water runs out of the ſecond to re-fill the firſt, by a tube furniſhed with a braſs cock, which greatly facilitates the filling of the principal boiler. The wooden covers of theſe boilers, which are double and movable on hinges, are ſhut down in grooves in which there is water, and the ſteam being by theſe means confined, is forced to paſs off by a wooden tube ſtanding on a part of the cover which is faſtened down to the boiler with hooks, carries the ſteam upwards to the height of ſeven or eight feet, where it goes off laterally by another (horizontal) wooden tube, through the wall into the drying-room. As ſoon as this horizontal wooden tube has paſſed through the wall into the drying-room, it ends in a copper tube, about three inches in diameter, which, lying nearly in a horizontal poſition, conducts the ſteam through the middle of the drying-room in the direction of its length, and through a hole in a window at the end of the room into the open air.

The ſteam in paſſing through the drying-room in a metallic tube, (which is a good conductor of heat,) gives off its heat through the ſides of the

tube

tube to the air of the room, and the water which is condenſed runs off through the tube. By ſloping the tube *upwards*, inſtead of downwards, as by accident it was ſlopped, the condenſed water, which is always nearly boiling-hot, when it is condenſed, might be made to return into the boiler, which would be attended with a ſaving of heat, and conſequently of Fuel.

The furnace for heating the irons uſed in ſmoothing the linen, (or ironing, as it is called,) is a kind of oven built of bricks and mortar, the bottom of which is a ſhallow pan of caſt iron, 18 inches ſquare and about 3 inches deep, which is nearly filled with fine ſand. The fire being made under this pan in a cloſed fire-place, as the ſand defends the upper ſurface of the pan from the cold air of the atmoſphere, the pan is commonly red-hot, and the irons, being ſhoved down through the ſand and placed in contact with this plate of red-hot metal, are heated in a very ſhort time, and at a ſmall expence of Fuel.

This contrivance might be uſed with great ſucceſs for covering the *hot plates* on which ſauce-pans are made to boil in many private Kitchens.

This ſtove, or oven, for heating the ſmoothing-irons, projects into the drying-room, but the door by which the irons are introduced, as well as that leading to the fire-place, and that leading to the aſh-pit, all open into the ironing-room.

The ſmoke goes off through the drying-room in an iron tube, and aſſiſts in warming the room and in drying the linen.

As it may ſometimes be neceſſary to heat the drying-room when neither the waſh-houſe boilers nor the ſtove for heating the ſmoothing-irons are heated, proviſion is made for that, by conſtructing a ſmall cloſed fire-place, deſigned merely for that purpoſe, which opens into the flue, by which the ſmoke from the boilers is carried round the drying-room. This fire-place (which is never uſed but when it is wanted for drying the linen) is ſituated juſt without the drying-room, under the end of the flue where it joins the ſecond boiler.—The opening at the top of its fire-place, by which the flame of the burning Fuel enters the under part of the flue, is kept cloſed by a ſliding plate of iron, when this fire-place is not uſed; and when it is uſed, the door which cloſes the opening into the fire-place of the firſt or principal boiler, and the regiſter in its aſh-pit door, are both kept ſhut.

That the top of the principal boiler might not be too high above the pavement of the waſh-houſe for the laundreſſes to work in the boiler without being obliged to go up ſteps or ſtairs, the grate and the bottom of the flues under the boiler are nearly on a level with the pavement, and the aſh-pit is ſunk into the ground, and to render the approach to the opening into the fire-place more convenient in introducing the Fuel, and lighting and managing the Fire, there is an area before the fire-place, about 3 feet ſquare, and 2 feet deep, ſunk in the ground, and walled up on its ſides, into which there is a deſcent by ſteps. In two of the ſides of theſe vertical walls (thoſe on the right and left when you ſtand fronting the fire-place) there are vaults for containing Fuel, which extend ſeveral feet under the pavement. The ſteps which deſcend into this area are on the ſide of it oppoſite the fire-place.

Areas of this kind are very neceſſary for all fire-places for large boilers, otherwiſe the top of the boiler will neceſſarily be raiſed too high above the level of the pavement to be approached with facility and convenience. Steps may be made, it is true, for approaching boilers which are placed higher; but theſe are always inconvenient, and take up more room, and coſt more than the execution of the plan here propoſed for rendering them unneceſſary.

The areas before the fire-place door of the large boilers in the Kitchen of the Foundling Hoſpital are occaſionally cloſed by trap-doors. As often as this is done there muſt be a number of ſmall holes bored in the door, to permit the air neceſſary for feeding the Fire to deſcend into the aſh-pit; and when the bottom of the paſſage leading into the fire-place happens to lie above the level of the upper ſurface of this trap-door, the part of the door immediately under this opening ſhould, to prevent accidents from live coals which may occaſionally fall out of the fire-place, be covered with a thin plate of ſheet-iron.

When large boilers are fitted up in ſituations where it is not poſſible to ſink an area in front of the fire-place, the maſs of brick-work in which the boiler is ſet muſt be raiſed, and ſteps muſt be made to approach it. When this is done, the upper ſtep ſhould be made very wide, (at leaſt two feet,) in order that there may be room to ſtand and work in the boiler; and for ſtill greater convenience, the ſteps ſhould be continued round three ſides of the boiler, when the boiler ſtands in a detached maſs of brick-work. The bottom of the door of the fire-place ſhould, if poſſible, be above the upper flat ſurface of the upper ſtep; and to preſerve the ſymmetry of the whole, the aſh-pit door may be in the front of the upper ſtep, and the paſſage into the aſh-pit (which will be long of courſe) may deſcend in a gentle ſlope. In this manner the Kitchen of the Hoſpital of *la Piéta* at *Verona* was conſtructed.

No inconvenience whatever attends the increaſe of the length of the paſſage into the aſh-pit, except it be that very trifling one,—(which ſurely does not deſerve to be mentioned,)—the increaſe of labour attending the removal of the aſhes; but the inconvenience would be very conſiderable which would unavoidably attend the diſcontinuation, or breaking off, of the ſteps round the hither end, or front of the boiler, which would be neceſſary in order to be able to place the aſh-pit door *directly under* the fire-place door, and to make a way to approach it.

The flues under the principal boiler of the Laundry in the Houſe of the Dublin Society, are not contrived ſo as to divide the flame and cauſe it to circulate in *two* currents; they run from ſide to ſide under it: the door of the fire-place is not in the middle, but on one ſide of the boiler, and near one end of it. The flame paſſing and returning (under the boiler) twice from its front to its oppoſite ſide, goes off at its end (that fartheſt from the fire-place) into a canal furniſhed with a damper, which canal, riſing upward at an angle of about 45 degrees, leads to the flues under the ſecond boiler.

The bottom of the flues of the principal boiler are juſt on a level with the pavement of the waſh-houſe; and, in order that they may eaſily be cleaned out, and the bottom of the boiler ſcrubbed with a broom to free it from ſoot, the ends of the flues were, in building the fire-place, left open, and afterwards, when the boiler was ſet, they were cloſed by temporary (double) walls of dry bricks. To make theſe walls tight, the joinings of the bricks were plaſtered on the outſide with moiſt clay.

The ſides of the boilers are defended from the cold air by thin walls of bricks covered with wainſcot, and by filling the ſpace between theſe walls and the boiler with pounded charcoal. Were I to fit up theſe boilers again, I ſhould leave this ſpace void, or filled merely with air, forming ſeveral ſmall openings below, through which the flame and hot vapour from the flues might aſcend and ſurround the boiler. In the large Boiler fitted up in the Linen-hall as a model for bleachers, this alteration is alſo neceſſary, to render it complete; and as it might be made in a few hours, and almoſt without any expence, I cannot help expreſſing a wiſh that it might ſtill be done.

The ardent zeal for the proſperity of his country, and indefatigable attention to every thing that tends to promote uſeful improvement, which ſo eminently diſtinguiſh that enlightened patriot and moſt reſpectable ſtateſman, to whom the manufactures and commerce of Ireland, and the linen-trade in particular, are ſo much indebted, encourage me to hope that he will take pleaſure in giving his aſſiſtance to render the models for improving fire-places and ſaving Fuel, which I have had the ſatiſfaction of leaving in Ireland, as free from faults as they can poſſibly be made.

Though my ſtay in Ireland was too ſhort to conſtruct models of all the improvements I wiſhed to have introduced in that delightful and moſt intereſting iſland, yet the liberality with which my various propoſals were received, and the generous aſſiſtance I met with from all quarters, enabled me to do more in two months than I probably ſhould have been able to have effected in as many years in ſome other older countries, where the progreſs of wealth and of refinements have rendered it extremely difficult to get people to attend to uſeful improvements.

I wiſhed much to have been able to have fitted up the great Kitchen in the Houſe of Induſtry at Dublin, as the expence of Fuel is very conſiderable in that extenſive eſtabliſhment, where more than 1500 perſons are fed daily, at an average; but not having time to finiſh ſo conſiderable an undertaking, I thought it moſt prudent not to begin it. I fitted up one large boiler as a model at one end of one of the working-halls; but this was deſigned principally to ſhow how a large hall might be heated from a Kitchen fire-place, and from the very ſame Fire which is uſed for cooking*. The ſmoke from the fire-place is carried along horizontally on one ſide of the hall from one end of it to the other; and the boiler being cloſed by a cover which is ſteam-tight, the ſteam from the boiler is alſo forced along from one end of the hall to the other, in an horizontal leaden pipe, which runs parallel to the flue occupied by the ſmoke, and lies immediately over it. In warm weather, when the hall does not require to be heated, the ſmoke and ſteam go off immediately into the atmoſphere by a chimney adjoining to the fire-place, without paſſing through the hall.

To be able to equalize the heat in the hall,—(which is very long and narrow,)—or to render it as warm at the end of it which is fartheſt from the fire-place, as at that next the Fire, I directed clothing for the ſteam tube of warm blanketing to be made in lengths of three or four feet, to be occaſionally put round it and faſtened by buttons.

By clothing or covering the ſteam tube more or leſs, as may be found neceſſary in thoſe parts of the hall where the heat is greateſt, the ſteam being by this covering prevented from giving off its heat to the air through the tube, will go on farther and warm thoſe parts of the hall which otherwiſe would be not ſufficiently heated. The ſteam tube, which is conſtructed of very thin ſheet-lead, is about 3 inches in diameter, and inſtead of being laid exactly in a horizontal poſition, ſlopes a little upwards, juſt ſo much that the water which reſults from the condenſation of the ſteam may return into the boiler *.

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* This contrivance might eaſily be applied to the heating of hot-houſes, even though the hot-houſe ſhould happen to be ſituated at a conſiderable diſtance from the Kitchen.

The horizontal flue through which the ſmoke paſſes is a round tube of ſheet-iron, about 7 inches in diameter, divided, for the facility of cleaning it, in lengths of 12 or 15 feet, fixed nearly horizontally at different heights from the floor, or, in an interrupted line, in hollow pilaſters or ſquare columns of brick-work. A common hot-houſe flue, conſtructed of bricks and mortar, would have anſwered

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* I contrived a fire-place for heating one of the principal churches in Dublin on theſe principles, with ſteam (but without making uſe of the ſmoke); and I promiſed to give a plan (which, I am aſhamed to ſay, I have not yet been able to finiſh) for heating the ſuperb new building deſtined for the meeting of the Iriſh Houſe of Commons.

One of the two chimney fire-places, which I fitted up in the hall in which the meetings of the Royal Iriſh Academy are held, will, I imagine, be found to anſwer very well for heating high rooms and large halls in private houſes. In this fire-place I have endeavoured, and I believe ſucceſsfully, to unite the advantages of an open fire with thoſe of a German ſtove. The grate uſed in fitting up this fire-place, and which is of caſt iron, and far from being unelegant in its form, and which coſt only *ſeven ſhillings and ſixpence ſterling*, is decidedly the beſt adapted for open chimney fire-places, where coals are uſed as Fuel, of any I have yet ſeen. By a letter I lately received from a friend in Ireland, I had the ſatisfaction to learn that theſe grates are coming very faſt into general uſe in that country.

ſwered equally well for warming the hall, but would have taken up too much room, which is the only reaſon it was not preferred to theſe iron tubes.

In conſtructing the boiler, (which is of thin ſheet-iron,) I made an Experiment which ſucceeded even beyond my expectation. The flues under the boiler—(and there are none round it)—are projections from the bottom of the boiler;—they are hollow walls of ſheet-iron, about 9 inches high, and an inch and three quarters thick, into which the liquid in the boiler deſcends, and which in fact conſtitute a part of the boiler. By this contrivance the flame is ſurrounded on all ſides, except at the bottom of the flues, (where the heat has little or no tendency to paſs,) by the liquid which is heated, and the fire-place is merely a flat maſs of brick-work. The grate is even with the upper ſurface of this maſs of brick-work, and the aſh-pit is the only cavity in it.

In conſtructing the boiler, proviſion was made, by omitting or interrupting the hollow walls or diviſions of the flues, in the proper places, to leave room for introducing the Fuel,—for the paſſage of the flame from one flue to another,—and from the laſt flue into the canal by which the ſmoke goes off into the chimney, or into the iron tubes by which the hall is occaſionally warmed.

One principal object which I had in view in this Experiment was to ſee if I could not contrive a boiler, which, being ſuſpended under a waggon or other wheel carriage, might ſerve for cooking for troops on a march; or which, being merely ſet down on the ground, a fire might be immediately kindled under it.

Thoſe who will take the trouble to examine the boiler in queſtion, will find that the principle on which it is conſtructed may eaſily be applied to the objects here mentioned. But it is not merely for portable boilers that this conſtruction would be found uſeful; I am convinced that it would be very advantageous for the boilers of ſteam-engines,—for diſtilleries,—and for various other purpoſes. As the eſcape of heat into the brick-work is almoſt entirely prevented, and as the ſurface of the boiler on which the heat is made to act is greatly increaſed by means of the hollow walls, the liquid in the boiler is heated in a very ſhort time, and with a ſmall quantity of Fuel.

There is ſtill another advantage attending this conſtruction, which renders it highly deſerving the attention of diſtillers. By making the tops of the flues arched inſtead of flat, (which may eaſily be done, and which is actually done in the boiler in queſtion,) or in the form of the roof of a houſe, as the hotteſt part of the flame will of courſe always occupy the upper part of the flues, and as the thick or viſcous part of the liquor in the boiler, that which is in moſt danger of being burned to the bottom of the boiler, and giving a bad taſte to the ſpirit which comes over, cannot well lie on the convex or ſloping ſurface of theſe flues, there will be leſs danger of an accident which diſtillers have hitherto found it extremely difficult to prevent.

In conſtructing boilers on theſe principles for diſtillers, it will probably be found neceſſary to increaſe very much the thickneſs of the hollow walls of the flues; and perhaps to make them even deeper than the level of the bottom of the flues, in order more effectually to prevent the thick matter which will naturally ſettle in thoſe cavities from being expoſed to too great a heat.

A ſimilar advantage will attend large boilers conſtructed on theſe principles for making thick ſoups for hoſpitals; theſe ſoups being very apt to burn to the bottoms of the boilers in which they are prepared.

I made another Experiment in the Houſe of Induſtry in Dublin, which I wiſhed much to have had time to have proſecuted farther. Finding that the expence for wheaten bread for the Houſe was very great, (amounting, in the year 1795, to no leſs than 3841 l. ſterling,) I ſaw that a very conſiderable ſaving might be made by furniſhing thoſe who were fed at the public expence with oaten cakes (a kind of bread to which they had always been uſed,) inſtead of rendering them dainty and ſpoiling them by giving them the beſt wheaten bread that could be procured, as I found had hitherto been done. But to be able to furniſh oaten cakes in ſufficient quantities to feed 1500 perſons, ſome more convenient method of baking them than that commonly practiſed was neceſſary, and one in which the expence of Fuel might be greatly leſſened.

With a view to facilitate this important change in the mode of feeding the numerous objects of charity, and of *correction*, who were ſhut up together within the walls of that extenſive eſtabliſhment, I conſtructed what I would call a *Perpetual Oven.*

In the centre of a circular, or rather cylindrical maſs of brick-work, about eight feet in diameter, which occupies the middle of a large room on the ground floor, I conſtructed a ſmall circular cloſed fire-place for burning either wood, peat, turf, or coals. The diameter of the fire-place is about eleven inches, the grate being placed about ten inches above the floor, and the top of the fire-place is contracted to about four inches. Immediately above this narrow throat, ſix ſeparate canals (each furniſhed with a damper, by means of which its opening can be contracted more or leſs, or entirely cloſed) go off horizontally, by which the flame is conducted into ſix ſeparate ſets of flues, under ſix large plates of caſt iron, which form the bottoms of ſix ovens on the ſame level, and joining each other by their ſides, which are concealed in the cylindical maſs of the brick-work. Each of theſe plates of caſt iron being in the form of an equilateral triangle, they all unite in the centre of the cylindrical maſs of brick-work, conſequently the two ſides of each unite in a point at the bottom of it, forming an angle of ſixty degrees.

The flame, after circulating under the bottoms of theſe Ovens, riſes up in two canals concealed in the front wall of each Oven, and ſituated on the right and left of its mouth, and after circulating again in ſimilar flues on the upper flat ſurface of another triangular plate of caſt iron which forms the top of the Oven, goes off upwards by a canal furniſhed with a damper into a hollow place, ſituated on the top of the cylindrical maſs of the brick-work, from which it paſſes off in a horizontal iron tube, about ſeven inches in diameter, ſuſpended near the cieling of the room, into a chimney ſituated on one ſide of the room.

Theſe ſix Ovens, which are contiguous to each other in this maſs of brick-work, are united by their ſides by thin walls made of tiles about 1½ inches thick, and 10 inches ſquare, placed edgewiſe, and each Oven having its ſeparate canal, furniſhed with a regiſter communicating with the fire-place, any one or more of them may be heated without heating the others, or the heat may be turned off from one of them to the other in continual ſucceſſion; and, by managing matters properly, the proceſs of baking may be *uninterrupted.* As ſoon as the bread is drawn out of one of the Ovens, the fire may be immediately turned under it to heat it again, while that from under which the Fire is taken is filled with unbaked loaves, and cloſed up.

A principal object which I had in view in conſtructing this Oven was to prevent the great loſs of heat which is occaſioned in large Ovens, by keeping the mouth of the Oven open for ſo conſiderable a length of time as is neceſſary for putting in and drawing out the bread. As one of theſe ſmall Ovens contains only five large loaves, or cakes, it may be charged, or the bread when baked may be drawn, in a moment; and, during this time, the other five Ovens are kept cloſed, and conſequently are not loſing heat; *one* of them is heating, while the other *four* are filled with bread in different ſtages of the proceſs of baking.

When I conſtructed this Oven, though I had no doubt of its being perfectly well calculated for the uſe for which it was principally deſigned,—baking oaten cakes, which are commonly baked on heated iron plates,—yet I was by no means ſure it would anſwer for baking common bread in large thick loaves. I had not made the Experiment. And though I could not conceive that any thing more could be neceſſary in the proceſs of baking than *heat*,—and here I was abſolutely maſter of every degree of it that could poſſibly be wanted, and could even regulate the *ſucceſſion* of different degrees of it at pleaſure,—I thought it probable that ſome particular management might be required in baking bread in theſe metallic ovens, a knowledge of which could only be acquired by experience.

What ſerved to ſtrengthen theſe ſuſpicions was a diſcovery which had accidentally been made by the cook of the Military Academy. In the courſe of *his* experiments, he found that my Roaſter is admirably well calculated for baking pies, puddings, and paſtry of all kinds;—provided however that the Fire be managed *in a certain way;*—for when the Fire is managed in the ſame manner in which it ought to be managed in roaſting meat, pies and paſtry will abſolutely be ſpoiled. After repeated failures and diſappointments, and after having loſt all hopes of ever being able to ſucceed in his attempts, the cook (by mere accident, as he aſſured me) diſcovered the important ſecret;—and important he certainly conſiders it to be, and feels no ſmall degree of ſatisfaction,—not to ſay pride,—in having been ſo fortunate as to make the diſcovery. He muſt pardon me if I take the liberty,—even without his permiſſion,—to publiſh it to the world for the good of mankind.

The Roaſter muſt be well heated before the pies or paſtry are put into it, and the blowers muſt never be quite cloſed, during the proceſs.

I have lately found that by uſing ſimilar precautions, bread may be perfectly well baked in metallic Ovens, ſimilar to that in the Houſe of Induſtry in Dublin.

Thinking it more than probable that means might be deviſed for managing the heat in ſuch a manner as to perform that proceſs in Ovens conſtructed on theſe principles, and heated *from without;* and conceiving that not only a great ſaving of Fuel, but alſo ſeveral other very important advantages, could not fail to be derived from that diſcovery, on my return from Munich to England, in Auguſt laſt, I immediately ſet about making Experiments, with a view to the inveſtigation of that ſubject; and I have ſo far ſucceeded in them that, for theſe laſt four months, my table has been ſupplied entirely with bread baked in my own houſe, by my cook, in an Oven conſtructed of thin ſheet-iron, which is heated (like my Roaſters) from without;—and I will venture to add, that I never taſted better bread. All thoſe who have eaten of it have unanimouſly expreſſed the ſame opinion of it. It is very light,—moſt thoroughly baked without being too much dried,—and I think remarkably well-taſted. The loaves, which are made ſmall in order that they may have a greater proportion of cruſt, (which, when the bread is baked in this way, is ſingularly delicate,) are placed in the Oven on circular plates of thin ſheet-iron, raiſed about an inch on ſlender iron feet. Were the loaf placed on the bottom of the Oven, the under cruſt would preſently be burnt to a coal, and the bread ſpoiled. A precaution abſolutely neceſſary in baking bread in the manner here recommended, is to leave a paſſage for the ſteam generated in the proceſs of baking to eſcape. This may be done either by conſtructing a ſteam-chimney for that purpoſe, furniſhed with a damper; or ſimply by making a regiſter in the door of the Oven.

As this is not the proper place to enlarge on this ſubject, I ſhall leave it for the preſent; but I cannot help expreſſing a wiſh, that what I have here advanced may induce others, eſpecially *Bakers*, who may find their own advantage in the proſecution of theſe intereſting and important inveſtigations, to turn their attention to them.

How exceedingly uſeful would my Roaſters be, and Ovens conſtructed on the principles here recommended, on ſhipboard!—Having ſerved a campaign (as a volunteer) in a large fleet, (that commanded by Admiral Sir Charles Hardy in the year 1779,) and having made ſeveral long ſea-voyages, I have had frequent opportunities of ſeeing how difficult it is in bad weather to cook at ſea; and it is eaſy to imagine how much it would contribute to the comfort of ſea-faring people, eſpecially at times when they are expoſed to the greateſt fatigues and hardſhips, to enable them to have their tables well ſupplied with warm victuals.

In order that the motion of the veſſel might not derange any part of the apparatus uſed in the proceſs of cooking at ſea in my Roaſters, the form of the Roaſter ſhould be that of a perfect cylinder, and the dripping-pan in which the meat is placed ſhould be a longitudinal ſection of another cylinder, leſs in diameter than the Roaſter by about an inch, and ſuſpended on two pivots in the axis of the Roaſter, in ſuch a manner that the dripping-pan may ſwing freely in the Roaſter, without touching its ſides. The Roaſter ſhould be placed in the brick-work, with its axis in the direction of the length of the ſhip; and to prevent the gravy from being thrown out of the dripping-pan when the veſſel pitches, its hollow cavity ſhould be divided into a number of compartments, by partitions running acroſs it from ſide to ſide.

It remains for me to give ſome account of the Kitchen which I fitted up in the Houſe of the Dublin Society, as a model for private families;—and alſo of a Cottage Fire-place, and a Lime-kiln, which I conſtructed, as models for imitation, in the court-yard of that public building.

With regard to the Kitchen, it is neceſſary that I ſhould remark at ſetting out, that it was not intended ſo much to ſerve as a model complete of a convenient Kitchen for a private family, as to *diſplay a variety of uſeful inventions*, all or any of which may, at pleaſure, be eaſily adopted, in Kitchens of all kinds and of all dimenſions. I thought this would be more uſeful than any ſimple model of a Kitchen I could contrive.

It is however a very complete Kitchen; and though there are ſome contrivances belonging to it which might have been omitted, yet they will all, I am confident, be found uſeful for the different purpoſes for which they were particularly deſigned; and, in a Kitchen for a large family, would often come into uſe.

The general diſpoſition of the various parts of this Kitchen I conſider as being quite perfect. It is the ſame as that of the Hoſpital of *la Pièta* at *Verona;* and of a very complete private Kitchen which was built about two years ago at Munich, under my direction, in the houſe of BARON LERCHENFELD, Steward of the Houſehold to HIS

MOST SERENE HIGHNESS THE ELECTOR. In a future Eſſay, which will treat excluſively of the Conſtruction of Kitchen Fire-places, and of Kitchen Utenſils, I ſhall give a particular detailed account of the manner in which the various Boilers,—Steam-boilers,—Saucepans,—Oven,—Roaſters, &c.—are diſpoſed and connected in the maſs of brick-work in theſe Kitchens; and ſhall accompany theſe deſcriptions with a ſufficient number of Plates to render them perfectly intelligible.

Cottage Fire-place, and Iron Pot, for cooking for the Poor.

THE Cottage Fire-place which I fitted up as a model, in the court-yard of the Houſe of the Dublin Society, was not quite finiſhed when I left Ireland; but an idea may be formed from what was done of the general principles on which ſuch Fire-places may be conſtructed. On each ſide of the open chimney Fire-place, (which, being ſmall, was built in the middle of one much larger, which was conſtructed to repreſent a large open Fire-place, ſuch as are now general in Cottages,) I fitted up an Iron Pot on a peculiar conſtruction, caſt by Mr. Jackſon of Dublin, and deſigned for the uſe of a poor family in cooking their victuals. This Pot is nearly of a cylindrical form, about ſixteen inches in diameter, and eight inches deep; and under its bottom, which is quite flat, there is a thin ſpiral projection, which was caſt with the Pot, and ſerves inſtead of feet to it, the turns of which, when the Pot is ſet down on a flat ſurface, form a ſpiral flue in which the flame circulates under the bottom of the Pot. This projection, which is near half an inch thick where it is united with the bottom of the Pot, and leſs than a quarter of an inch below where its lower edge reſts on the ground, is about four inches wide, or rather deep. It was made tapering, in order to its being more eaſily caſt. To defend the outſide of this Pot from the cold air, the Pot is incloſed in a cylinder of thin ſheet-iron, equal in diameter to the extreme width of the Pot at its brim,—juſt as high as the depth of the Pot and of its ſpiral flues taken together. The Pot is faſtened to this cylindrical caſe by being driven into it with force, a rim in the form of a flat hoop, about an inch and an half deep and a little tapering, being caſt on the outſide of the Pot at its brim, the external ſurface of which was fitted exactly into the top of this cylinder. This projection is uſeful, not only in uniting the Pot to its cylindrical caſe, but alſo to keep this cylindrical caſe at ſome ſmall diſtance from the ſides of the Pot, by which means the heat is more effectually confined.

To be able to move about this Pot from place to place, it has two handles, which are riveted to the outſide of its cylindrical caſe; and it is provided with a wooden cover.

I am ſenſible that I often expoſe myſelf to criticiſm by anticipating what would more naturally find its place elſewhere. But what I have here ſaid in regard to this Iron Pot is intended merely as hints to awaken the curioſity and excite the attention of ingenious men,—of ſuch as take pleaſure in exerciſing their ingenuity in contriving and perfecting uſeful inventions; and who delight in contemplating the progreſs of human induſtry.

Model of a perpetual Lime-kiln.

THE particular objects principally had in view in the conſtruction of this Lime-kiln (which ſtands in the court-yard of the Dublin Society) were, *firſt*, to cauſe the Fuel to burn in ſuch a manner as to conſume the ſmoke;—which was effected by obliging the ſmoke to deſcend and paſs through the Fire, in order that as much heat as poſſible might be generated.—Secondly, to cauſe the flame and hot vapour which riſe from the Fire to come into contact with the lime-ſtone by a very large ſurface, in order to economiſe the heat, and prevent its going off into the atmoſphere; which was done by making the body of the Kiln in the form of a hollow truncated cone, and very high in proportion to its diameter; and by filling it quite up to the top with lime-ſtone, the Fire being made to enter near the bottom of the cone.—Thirdly, to make the proceſs of burning lime *perpetual*, in order to prevent the waſte of heat which unavoidably attends the cooling of the Kiln in emptying and filling it, when, to perform that operation, it is neceſſary to put out the Fire.—And fourthly, to contrive matters ſo that the lime in which the proceſs of burning is *juſt finiſhed*, and which of courſe is ſtill *intenſely hot*, may, in *cooling*, be made to give off its heat in ſuch a manner as to aſſiſt in heating the freſh quantity of cold lime-ſtone with which the Kiln is repleniſhed as often as a portion of lime is taken out of it.

To effectuate theſe purpoſes, the Fuel is not mixed with the lime-ſtone, but is burned in a cloſed fire-place, which opens into one ſide of the Kiln, ſome diſtance above the bottom of it. For large Lime-kilns on theſe principles there may be ſeveral fire-places, all opening into the ſame cone, and ſituated on different ſides of it; which fire-places may be conſtructed and regulated like the fire-places of the furnaces uſed for burning porcelaine, or earthen ware.

At the bottom of the Kiln there is a door, which is occaſionally opened to take out the lime.

When, in conſequence of a portion of lime being drawn out of the Kiln, its contents ſettle down, or ſubſide, the empty ſpace in the upper part of the Kiln, which is occaſioned by this ſubtraction of the burned lime, is immediately filled up with freſh lime-ſtone.

As ſoon as a portion of lime is taken away, the door by which it is removed muſt be immediately ſhut, and the joinings well cloſed with moiſt clay, to prevent a draught of cold air through the Kiln.

A ſmall opening however muſt be left, for reaſons which I ſhall preſently explain.

As the Fire enters the Kiln at ſome diſtance from the bottom of it, and as the flame *riſes* as ſoon as it comes into this cavity, the lower part of the Kiln (that below the level of the bottom of the fire-place) is occupied by lime already burned; and as this lime is intenſely hot, when, on a portion of lime from below being removed, it deſcends into this part of the Kiln, and as the air in the Kiln, to which it communicates its heat, muſt *riſe upwards* in conſequence of its being heated, and paſs off through the opening at the top of the Kiln, this lime in cooling is, by this contrivance, made to aſſiſt in heating the freſh portion of cold lime-ſtone with which the Kiln is charged. To facilitate this communication of heat from the red-hot lime juſt burned, to the lime-ſtone above, in the upper part of the Kiln, a gentle draught of air through the Kiln from the bottom to the top of it, muſt be eſtabliſhed, which is done by leaving an opening in the door below, by which the cold air from without may be ſuffered to enter the Kiln. This opening (which ſhould be furniſhed with ſome kind of a regiſter) muſt be very ſmall, otherwiſe it will occaſion too ſtrong a draught of cold air into the Kiln, and do more harm than good; and it will probably be found to be beſt to cloſe it entirely, after the lime in the lower part of the Kiln has parted with a certain proportion of its heat.

Conceiving the improvement of Lime-kilns to be a matter of very great national importance, eſpecially ſince the uſe of lime as manure has become ſo general, I intend to devote the firſt leiſure time I can ſpare to a thorough inveſtigation of that ſubject;—in the mean time, I have here thrown out the looſe ideas I have formed reſpecting it, in order that they may be examined, corrected, and improved upon by others who may be engaged in the ſame purſuits.

The model I cauſed to be conſtructed in the court-yard of the Dublin Society, is, I am ſenſible, very imperfect. It was built in a great hurry, being begun and finiſhed the ſame day,—the day but one before I left Ireland;—but I am now engaged in conſtructing a Lime-kiln on the ſame principles, (for the uſe of the farm in the Engliſh Garden at Munich,) which I ſhall take pains to make as perfect as poſſible; and ſhould it be found to anſwer as well as I have reaſon to hope it will, I ſhall not fail to give a particular account of it to the Public, accompanied with drawings, and all the details that ſhall be neceſſary in order to give the moſt ſatisfactory account of the reſult of the Experiment.

Theſe inveſtigations will be the more intereſting, and the reſults more generally uſeful, as the diſcovery of a mine of pit-coal in the neighbourhood of Munich, which is now worked with ſucceſs, has put it in my power to uſe coal as Fuel, as well as wood and turf, in the Experiments I ſhall make in burning lime in this Kiln.

For the information of thoſe who may be diſpoſed to engage in theſe purſuits, I have publiſhed the annexed ſketch of the Lime-kiln in queſtion, which is now actually building. (See Plate VI.) I thought it right to do this, that we might ſtart fair; and I can aſſure my competitors in this race, that I ſhall feel no ill-will on ſeeing them get before me.

If I do not deceive myſelf, the laudable exertions of others afford me almoſt as much pleaſure as my own purſuits;—at leaſt I am quite certain that when I can flatter myſelf that I have had any,—even the ſmalleſt ſhare,—in *exciting* thoſe exertions, the ſatisfaction I feel in contemplating them is inexpreſſible.

DESCRIPTION OF THE PLATES.

PLATE I.

FIG. 1. A view of a double cover for a boiler or ſaucepan. In this deſign the rim is ſeen which enters the boiler, and the tube by which the ſteam goes off is ſeen in part (above),—and is in part indicated by dotted lines. (See page 18.)

Fig. 2 ſhows this cover placed on its boiler. Part of the ſide of the cover is repreſented as wanting, in order that the ſteam tube might be better ſeen. The height of this cover is repreſented as being equal to *one-half* its diameter; but I have found *one-third*, or even *one-quarter* of its diameter to be quite ſufficient for its height.

Fig. 3 and Fig. 4 are views of my circular diſhing-grates for cloſed kitchen fire-places. They may be made of any ſize, from 5 inches to 18 inches in diameter, according to the ſize of the boiler. The rules I have in general followed, in determining the ſize proper for the grate for any (circular) boiler, has been to make its diameter equal to half the diameter of the boiler at the brim. (See page 41.)

Fig. 5 is an inverted hollow cone of thin ſheet-iron, which is placed immediately under the grate, its brim being made to receive the circular rim of the grate. When the fire-place is large, this inverted cone may be made of fire-ſtone, or it may be conſtructed of bricks and mortar. For ſmall fire-places it may be made of earthen ware, which is perhaps the very beſt material for it that can be found. (See page 43.)

Fig. 6, Fig. 7, and Fig. 8, are views and ſections of a perforated tile, with its ſtopper, ſuch as are uſed for cloſing the entrance by which the Fuel is introduced into cloſed kitchen fire-places. The diameter of the circular opening, or hole in the tile, may be from 6 to 7 inches. (See page 30.)

PLATE II.

The various Figures, from No. 9 to No. 16 of this Plate, ſhow the conſtruction of an aſh-pit door, with its regiſter. (See page 31.)

Fig. 9 is a front view of the door with its regiſter.—The whole is conſtructed of ſheet-iron, except the four narrow pieces at the four corners, which hold down in its place the circular plate of the regiſter; and the ſmall circular plate (as large as an half-crown) in the centre of the regiſter, which are made of braſs, on account of that metal not being ſo liable to ruſt, as iron.

Fig. 10 is a ſide view of the backſide of the door, fixed in its frame, in which the manner of its being ſhut in its frame is ſeen, and the iron ſtraps *a b c d* are ſeen, by which the frame is faſtened in the brick-work.

Fig. 11 is an horizontal ſection through the middle of the door and its frame, and through the button which ſerves for ſhutting the door.

Fig. 12 is a ſection of this button, on an enlarged ſcale, ſhowing the manner in which it is conſtructed.

Fig. 13 is the plate of ſheet-iron which forms the front of the door, with the holes in it, by which the other parts of the machinery are fixed to it.

Fig. 14 is the circular plate which forms the regiſter—to this plate is fixed a projecting knob, or button, (repreſented in the figure,) by which it is turned about.

Fig. 15 and Fig. 16 ſhow, on an enlarged ſcale, one of the four pieces of braſs by which the circular plate of the regiſter is kept down in its place.

In conſtructing theſe regiſter doors, and in general all iron doors for fire-places, great and ſmall, the door ſhould never ſhut in a rabbet, or groove, in the frame, but ſhould merely *ſhut down on the front edge of the frame*, which edge, by grinding it on the flat ſurface of a large flat ſtone, ſhould be made quite level to receive it. If this be done, and if the plate of iron which conſtitutes the door be made quite flat, and if it be properly fixed on its hinges, the door will always ſhut with facility and cloſe the opening with preciſion, notwithſtanding the effects of the expanſion of the metal by heat; but this cannot be the caſe when the doors of fire-places are fitted in grooves and rabbets.

Where the heat is very intenſe, the frame of the door ſhould be made of fire-ſtone; and that part of the door which is expoſed naked to the fire ſhould be covered, either with a fit piece of fire-ſtone, faſtened to it with clamps of iron, or a ſufficient number of ſtrong nails, with long necks and flat heads, or of ſtaples, being driven into that ſide of the plate of iron which forms the door which is expoſed, that ſide of it ſhould be covered with a body about two inches thick of ſtrong clay mixed with a due portion of coarſe powder of broken crucibles, which maſs will be held in its place by the heads of the nails, and by the projecting ſtaples. This maſs being put on wet, and gently dried, the cracks being carefully filled up as they appear, and the whole well beaten together into a ſolid maſs, will, when properly burned on by the heat of the fire, form a covering for the door which will effectually defend it from all injury from the fire; and the door ſo defended will laſt ten times longer than it would laſt without this defence.

The inſide doors of the two Brew-houſe Fire-places which I have fitted up at Munich are both defended from the heat in this manner; and the contrivance, which has anſwered perfectly all that was expected from it, has not been found to be attended with any inconvenience whatever.

PLATE

PLATE III.

Fig. 17 is a front view of the new boiler of the brew-houſe called *Neuheuſal*, or rather of its fire-place and cover (the boiler being concealed in the brick-work). The inſide door of the fire-place is here repreſented ſhut; and, in order that it might appear, the outſide door is taken off its hinges, and is not ſhown. The two vaulted galleries A B in the ſolid maſs of the brick-work, on the right and left of the fire-place, (which were made to ſave bricks,) ſerve for holding fire-wood. The partition walls of the fire-place and the different flues, as alſo a ſection of the boiler, are repreſented by dotted lines. The ſmall circular hole on the left of the fire-place door is the window opening into the fire-place, by which the burning Fuel may be ſeen.

a b is the wooden curb of the boiler: *c d* a platform on which the men ſtand when they work in emptying the boiler, &c.: *e f* is a platform which ſerves as a paſſage from one ſide of the boiler to the other. This platform, which is about 18 inches wide, is 12 inches higher than the other platforms, in order that the openings *g* and *h*, into the flues, may remain free. Theſe openings, which are opened only occaſionally, that is to ſay, when the flues want cleaning, are kept cloſed by double brick-walls. Theſe walls are expreſſed in the following Figure.

Fig. 18. This is a horizontal ſection of the fire-place at a level with the bottom of the boiler.

a a a a are four openings by which the flues, which, in the firſt arrangement of this fire-place, went round the outſide of the boiler, was occaſionally cleaned: *b* is the canal by which the ſmoke went off into the chimney.

The entrance into the fire-place, and the conical perforation in the wall of the fire-place which ſerves as a window for obſerving the fire, are marked by dotted lines. The poſition of the inſide door of the fire-place is marked by a dotted line, *c d.* The circular diſhing-grate is ſeen in its place; and the walls of the flues under the boiler are all ſeen. The crooked arrows in the flues ſhow the direction of the flame. (See page 100.)

PLATE IV.

Fig. 19 is a vertical ſection of the boiler repreſented in the foregoing Plate (Fig. 17.). This ſection is taken through the middle of the boiler,—of the fire-place;—and of the cover of the boiler. A is the aſh-pit, with a ſection of its regiſter door. B is the fire-place, and its circular diſhing-grate. C is the entrance by which the Fuel is introduced, with ſections of its two doors. D is a ſpace left void to ſave bricks. E is the boiler, and F its wooden cover. *m* is the ſteam chimney, which is furniſhed with a damper. R R is the vertical wall of the houſe againſt which the brick-work in which the boiler is fixed is placed.

a b is the curb of timber in which the boiler is ſet.

The manner in which the cover of the boiler is conſtructed, as well as its form, and the door and windows which belong to it, are all ſeen diſtinctly in this Figure.

Fig. 20 is an horizontal ſection of this fire-place taken on a level with the bottom of the flue which goes round the outſide of the boiler; in which flue, before the fire-place was altered, the flame circulated. The flues under the boiler are, in this Figure, indicated by dotted lines.

PLATE V.

Fig. 21 is an horizontal ſection of the fire-place of the brewhouſe boiler, at a level with the top of the flues under the boiler, *after the flue round the outſide of the boiler had been ſtopped up*, or rather the flame prevented from circulating in it. This Figure ſhows the actual ſtate of the fire-place at the preſent time. (See page 126.)

The crooked arrows ſhow the direction of the flame in the flues—*a b* are the two canals (each of which is furniſhed with a damper) by which the ſmoke goes off into the chimney;—and *c c c c c c* are ſix ſmall openings communicating with the flues, by which the flame and hot vapour can paſs up into the cavity on the outſide of the boiler which formerly ſerved as a flue.

Fig. 22 is a front view of the aſh-pit door of this brewhouſe fire-place, with its regiſter. This door is cloſed by means of a latch of a particular conſtruction, which is ſhown in the Figure.

Fig. 23 is the door without its regiſter;—and

Fig. 24 the circular plate of the regiſter repreſented alone.

This aſh-pit door ſhuts againſt the front edge of its frame, and not into it; the reaſons for preferring this method of fitting the door to its frame have already been explained. (See deſcriptions of the Plate II.)

PLATE VI.

Fig. 25 is a ſection of a ſmall lime-kiln, built, or rather now building, at Munich, for the purpoſe of making experiments. The height of the kiln is 15 feet;—its internal diameter below, 2 feet,—and above, 9 inches. In order more effectually to confine the heat, its walls, which are of bricks and very thin, are double, and the cavity between them is filled with dry wood aſhes. To give greater ſtrength to the fabrick, theſe two walls are connected in different places by horizontal layers of bricks which unite them firmly.

a is the opening by which the fuel is put into the fire-place. Through this opening the air *deſcends* which feeds the fire. The fire-place is repreſented nearly full of coals, and the flame paſſing off laterally into the cavity of the kiln, by an opening made for that purpoſe at the bottom of the fire-place.

The opening above, by which the fuel is introduced into the fire-place, is covered by a plate of iron, moveable on hinges; which plate, by being lifted up more or leſs by means of a chain, ſerves as a regiſter for regulating the fire.

A ſection of this plate, and of the chain by which it is ſupported, are ſhown in the Figure.

b is an opening in the front wall of the fire-place, which ſerves occaſionally for cleaning out the fire-place, as alſo for cleaning out the opening by which the flame paſſes from the fire-place into the kiln. This opening, which muſt never be quite cloſed, ſerves likewiſe for admitting a ſmall quantity of air to paſs horizontally into the fire-place. A ſmall proportion of air admitted in this manner has been found to be uſeful, and even neceſſary, in fire-places in which, in order to conſume the ſmoke, the flame is made to deſcend. Several ſmall holes for this purpoſe, fitted with conical ſtoppers, may be made in different parts of the front wall of the fire-place.

The bottom of the fire-place is a grate, conſtructed of bricks placed edgewiſe, and under this grate there is an aſh-pit; but as no air muſt be permitted to paſs up through this grate into the fire-place the aſh-pit door, *c* is kept conſtantly cloſed being only opened occaſionally to remove the aſhes.

d is the opening by which the lime is taken out of the kiln; which opening muſt be kept well cloſed, in order to prevent a draught of cold air through the kiln.

As only as much lime muſt be removed at once as is contained in that part of the kiln which lies below the level of the bottom of the fire-place, to be able to aſcertain when the proper quantity is taken away, the lime, as it comes out of the kiln, may be directed into a pit ſunk in the ground in front of the opening by which the lime is removed, this pit being made of proper ſize to ſerve as a meaſure.

While the lime is removing from the bottom of the kiln, freſh lime-ſtone ſhould be put into it above; and, during this operation, the fire may be damped by cloſing the top of the fire-place with its iron-plate.

Should it be found neceſſary, the fire, and the diſtribution of the heat, may, in burning the lime, be farther regulated by cloſing more or leſs the opening at the top of the lime-kiln with a flat piece of fireſtone, or a plate of caſt iron.

The double walls of the kiln, and the void ſpace between them, as alſo the horizontal layers of bricks by which they are united, are clearly and diſtinctly expreſſed in the Figure. The kiln is repreſented as being nearly filled with ſmall round ſtones, ſuch as are uſed at Munich in burning lime. Theſe ſtones are brought down from the calcareous mountains on our frontiers, by the river (the Iſar), and are rounded by rubbing againſt each other as they are rolled along by the impetuoſity of the torrent.

END OF THE SIXTH ESSAY.

Plate 1.

Fig. 1.

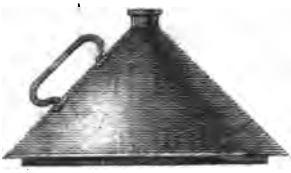

Fig. 2.

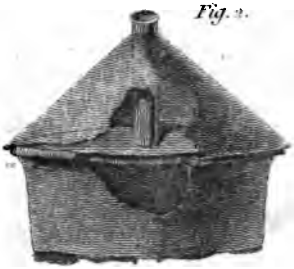

Fig. 3.

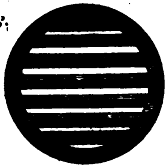

Fig. 4.

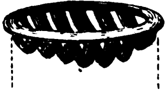

Fig. 5.

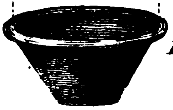

Fig. 6.

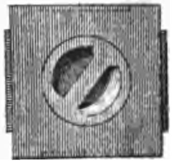

Fig. 7.

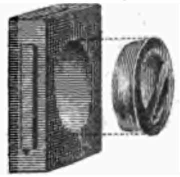

Fig. 8.

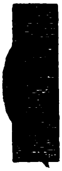

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Fig. 9.

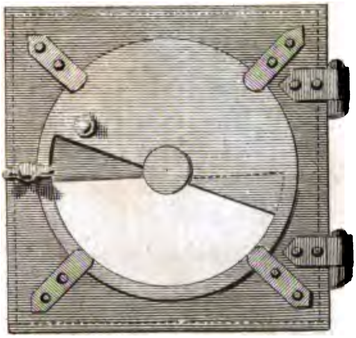
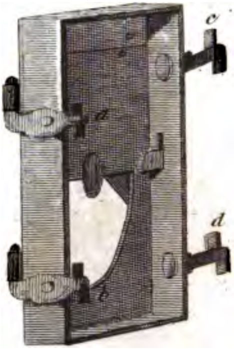


Fig. 10.



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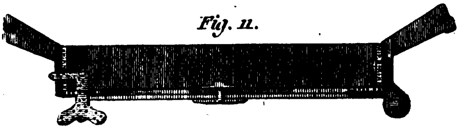
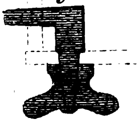


Fig. 12.



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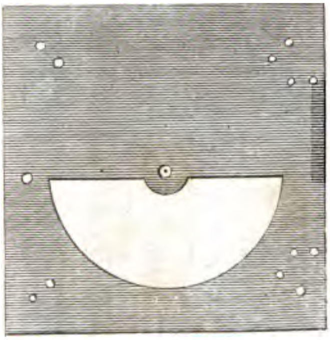

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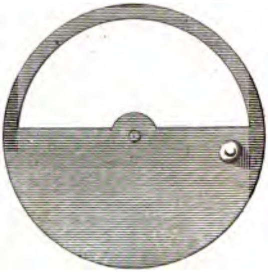

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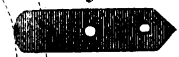

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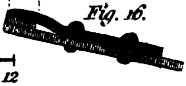

Scale of Inches

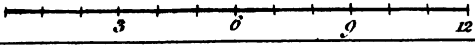

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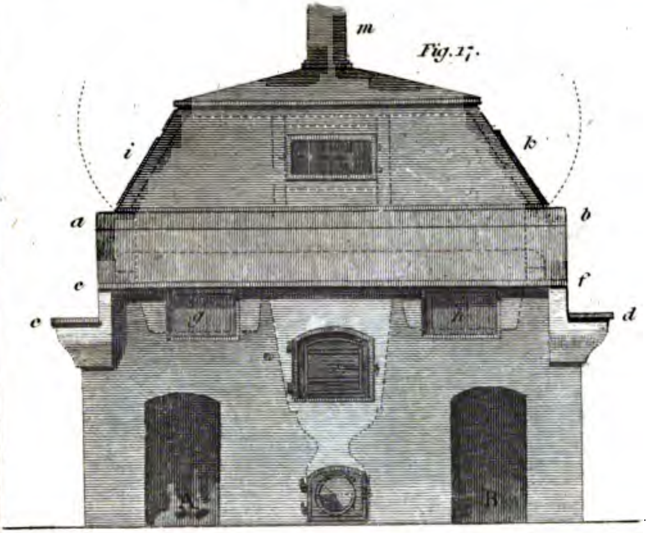
Fig. 17.



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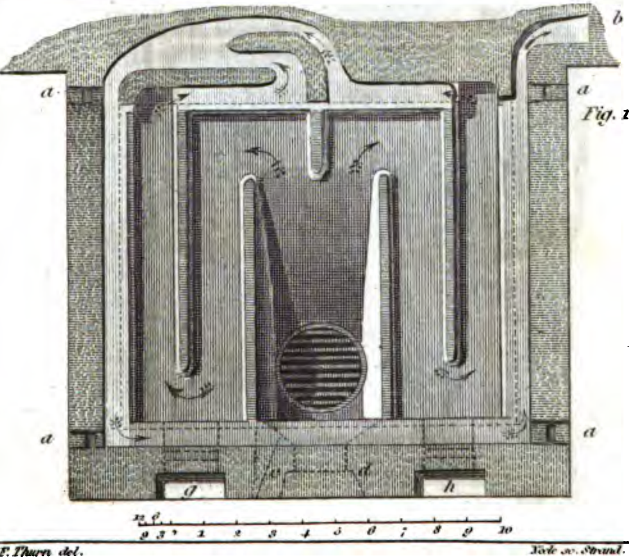

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Plate 4.

Fig. 19.

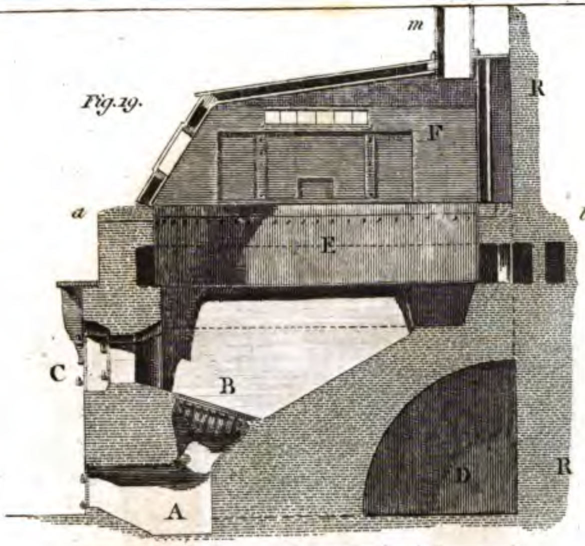

Fig. 20.

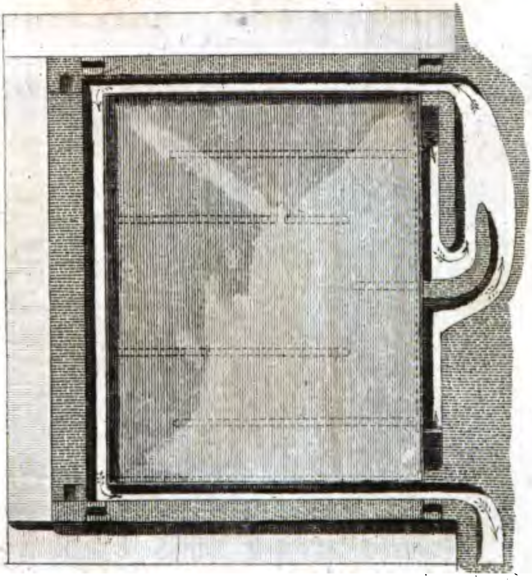

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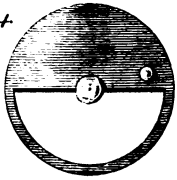
Plate 3.

Fig. 24.



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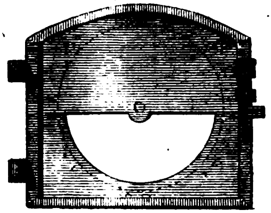

Fig. 22.

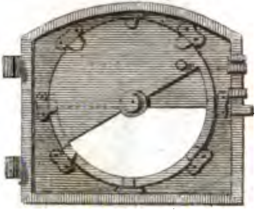

Fig. 21.

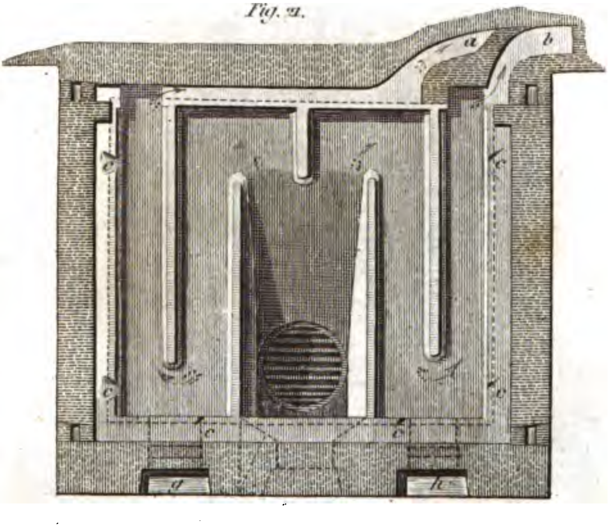

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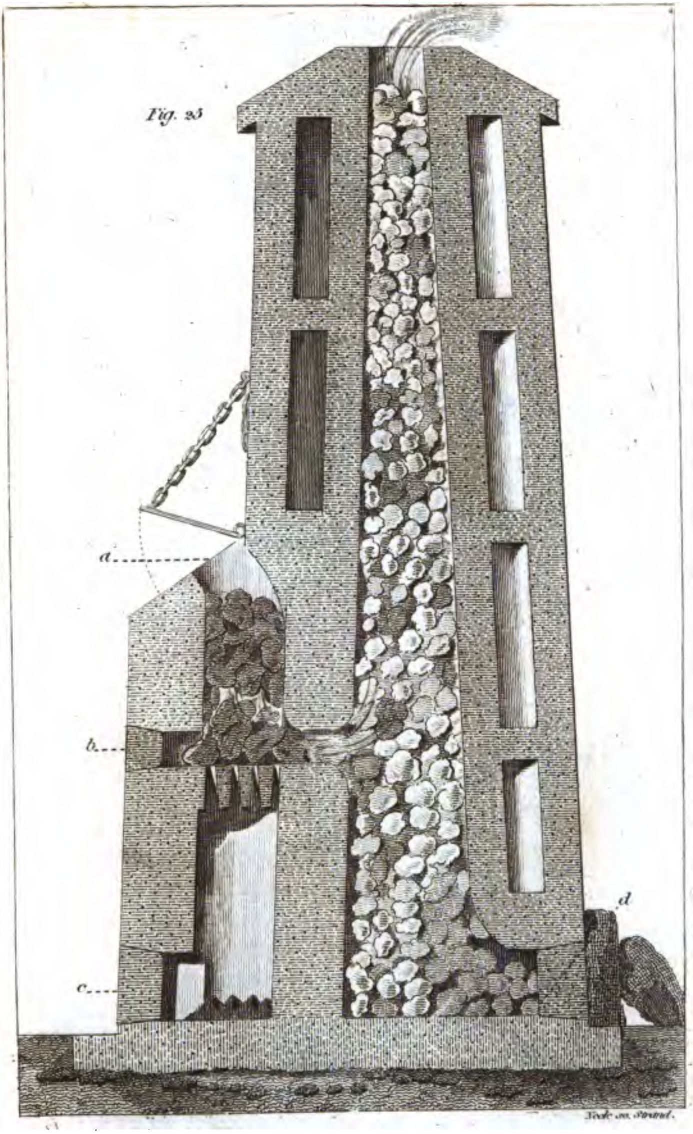
Plate 8.

Fig. 25



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ESSAY VII.

OF

THE PROPAGATION OF HEAT IN FLUIDS.

IN TWO PARTS.

ESSAY VII.

PART I.

Of a remarkable LAW which has been found to obtain, in the Condenſation of WATER with COLD, when it is near the Temperature at which it freezes; and of the wonderful Effects which are produced by the Operation of that LAW, in the Economy of Nature. Together with Conjectures reſpecting the FINAL CAUSE of the SALTNESS OF THE SEA.

CHAP. I.

Danger of admitting received Opinions in Philoſophical Inveſtigations, without Examination.—The free Paſſage of HEAT, *in all Bodies, in all Directions, never yet called in queſtion.—Heat does not, however, paſs in this Manner, in all Bodies without Exception.*—AIR *and* WATER, *and probably all other* FLUIDS, *are, in fact,* NON-CONDUCTORS OF HEAT.—*Accidental Diſcoveries, which led to an experimental Inveſtigation of this curious Subject.—The internal Motions among the Particles of Fluids rendered viſible.—The Propagation of Heat in Fluids obſtructed and retarded, by every thing which obſtructs the internal Motions of their Particles;—hence there is Reaſon to conclude, that Heat* *is propagated in them, only in conſequence of thoſe Motions;—that it is tranſported by them,—not ſuffered to paſs through them.—*FURS *and* FEATHERS, *and all other like Subſtances, which, in Air, form warm Covering for confining Heat, found, by Experiment, to produce the ſame Effects in Water.—Theſe Effects are probably produced in both Fluids in the ſame Manner, namely, by obſtructing the Motions of their Particles, in the Operation of tranſporting the Heat.—The conducting Power of Water, remarkably impaired by mixing with it ſuch Subſtances as render it viſcous, and diminiſh its Fluidity.—Theſe Diſcoveries reſpecting the Manner in which Heat is propagated in Water, throw much Light on ſeveral of the moſt intereſting Operations in the Economy of Nature.—They enable us to account, in a ſatisfactory Manner, for the Preſervation of Trees and other Vegetables, and of Fruits, during the Winter, in cold Climates.*

IT is certain, that there is nothing more dangerous, in philoſophical inveſtigations, than to take any thing for granted, however unqueſtionable it may appear, till it has been proved by direct and deciſive experiment.

I have very often, in the courſe of my philoſophical reſearches, had occaſion to lament the conſequences of my inattention to this moſt neceſſary precaution.

There is not, perhaps, any phænomenon that more frequently falls under our obſervation, than the Propagation of Heat. The changes of the temperature of ſenſible bodies—of ſolids—liquids—and elaſtic fluids, are going on perpetually under our eyes; and there is no fact which one would not as ſoon think of calling in queſtion, as to doubt of the free paſſage of Heat, in all directions, through all kinds of bodies. But, however obviouſly this concluſion appears to flow, from all that we obſerve and experience in the common courſe of life, yet it is certainly not true;—and to the erroneous opinion reſpecting this matter, which has been univerſally entertained—by the *learned*, and by the *unlearned*—and which has, I believe, never even been called in queſtion, may be attributed the little progreſs that has been made in the inveſtigation of the ſcience of Heat: a ſcience, aſſuredly, of the utmoſt importance to mankind!

Under the influence of this opinion, I, many years ago, began my experiments on Heat; and had not an accidental diſcovery drawn my attention with irreſiſtible force, and fixed it on the ſubject, I probably never ſhould have entertained a doubt of the free paſſage of Heat *through air;* and even after I had found reaſon to conclude, from the reſults of experiments which to me appeared to be perfectly deciſive, that air is a *non-conductor* of Heat; or that Heat cannot paſs through it, without being tranſported by its particles; which, in this proceſs, act individually, or independently of each other; yet, ſo far from purſuing the ſubject, and contriving experiments to aſcertain the manner in which Heat is communicated in other bodies, I was not ſufficiently awakened to ſuſpect it to be even poſſible, that this quality could extend farther than to elaſtic fluids.

With regard to liquids, ſo entirely perſuaded was I, that Heat could paſs freely, *in them*, in all directions, that I was perfectly blinded by this prepoſſeſſion, and rendered incapable of ſeeing the moſt ſtriking and moſt evident proofs of the fallacy of this opinion.

I have already given an account, in one of my late publications—(Eſſay VI. on the Management of Fire, and the Economy of Fuel)—of the manner in which I was led to diſcover, that *ſteam* and *flame* are *non-conductors* of Heat:—I ſhall now lay before the Public an account of a number of experiments I have lately made, which ſeem to ſhow that *water*,— and probably all other liquids,—and Fluids of every kind, poſſeſs the ſame property. That is to ſay, that although the particles of any Fluid, *individually*, can receive Heat from other bodies, or communicate it to them; yet among theſe particles themſelves, all *interchange* and *communication* of Heat is abſolutely impoſſible *.

It may, perhaps, be thought not altogether uninteresting, to be acquainted with the various ſteps by which I was led to an experimental inveſtigation of this curious ſubject of inquiry.

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* I do not pretend to determine the cauſe of this phyſical impoſſibility. Perhaps it may be owing merely to the extreme mobility of the integrant particles, or molecules of Fluids. My reſearches have all been confined to the Diſcovery of the Fact, and I cheerfully leave to others the inveſtigation of its cauſes. [This Note was firſt introduced in the third Edition of this Eſſay, publiſhed in the month of March 1800.]

When dining, I had often obſerved that ſome particular diſhes retained their Heat much longer than others; and that apple-pies, and apples and almonds mixed;—(a diſh in great repute in England,)—remained hot a ſurprizing length of time.

Much ſtruck with this extraordinary quality of retaining Heat, which apples appeared to poſſeſs, it frequently occurred to my recollection; and I never burnt my mouth with them, or ſaw others meet with the ſame misfortune, without endeavouring, but in vain, to find out ſome way of accounting, in a ſatiſfactory manner, for this ſurpriſing phænomenon.

About four years ago, a ſimilar accident awakened my attention, and excited my curioſity ſtill more: being engaged in an experiment which I could not leave, in a room heated by an iron ſtove, my dinner, which conſiſted of a bowl of thick rice-ſoup, was brought into the room; and as I happened to be too much engaged at the time to eat it, in order that it might not grow cold, I ordered it to be ſet down on the top of the ſtove: about an hour afterwards, as near as I can remember, beginning to grow hungry, and ſeeing my dinner ſtanding on the ſtove, I went up to it, and took a ſpoonful of the ſoup, which I found almoſt cold, and quite thick. Going, by accident, deeper with the ſpoon the ſecond time, this ſecond ſpoon-full burnt my mouth*. This accident recalled very forcibly to my mind the recollection of the hot apples and almonds, with which I had ſo often burned my mouth, a dozen years before in England; but even this, though it ſurpriſed me very much, was not ſufficient to open my eyes, and to remove my prejudices reſpecting the conducting power of water.

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* It is probable that the ſtove happened to be nearly cold when the bowl was ſet down upon it, and that the ſoup had grown almoſt cold; when, a freſh quantity of fuel being put into the ſtove, the Heat had been ſuddenly increaſed.

Being at NAPLES, in the beginning of the year 1794, among the many natural curioſities which attracted my attention, I was much ſtruck with ſeveral very intereſting phænomena which the hot baths of BAIA preſented to my obſervation; and among them there was one, which quite aſtoniſhed me: ſtanding on the ſea-ſhore, near the baths, where the hot ſteam was iſſuing out of every crevice of the rocks, and even riſing up out of the ground, I had the curioſity to put my hand into the water: As the waves which came in from the ſea followed each other without intermiſſion, and broke over the even ſurface of the beach, I was not ſurpriſed to find the water cold;—but I was more than ſurpriſed, when, on running the ends of my fingers through the cold water into the ſand, I found the heat ſo intolerable, that I was obliged inſtantly to remove my hand.

The ſand was perfectly wet; and yet, the temperature was ſo very different at the ſmall diſtance of two or three inches!—I could not reconcile this with the ſuppoſed great conducting power of water.—I even found that the top of the ſand was, to all appearance, quite as cold as the water which flowed over it; and this increaſed my aſtoniſhment ſtill more. I then, for the firſt time, *began to doubt of the conducting power of water*, and reſolved to ſet about making experiments to aſcertain the fact; I did not however put this reſolution into execution till about a month ago; and ſhould perhaps never have done it, had not *another* unexpected appearance again called my attention to it, and excited afreſh all my curioſity.

In the courſe of a ſet of experiments on the communication of Heat, in which I had occaſion to uſe thermometers of an uncommon ſize,—(their globular bulbs being above four inches in diameter,)—filled with various kinds of liquids, having expoſed one of them, which was filled with ſpirits of wine, in as great a heat as it was capable of ſupporting, I placed it in a window, where the ſun happened to be ſhining, to cool; when, caſting my eye on its tube, which was quite naked,—(the diviſions of its ſcale being marked in the glaſs with a diamond,)—I obſerved an appearance which ſurpriſed me, and at the ſame time intereſted me very much indeed. I ſaw the whole maſs of the liquid in the tube in a moſt rapid motion, running ſwiftly in two oppoſite directions, *up*, and *down*, at the ſame time.

The bulb of the thermometer, which is of copper, had been made two years before I found leiſure to begin my experiments; and having been left unfilled, without being cloſed with a ſtopple, ſome fine particles of duſt had found their way into it, and theſe particles, which were intimately mixed with the ſpirits of wine, on their being illuminated by the ſun's beams, became perfectly viſible,—(as the duſt in the air of a darkened room is illuminated and rendered viſible by the ſun-beams which come in through a hole in the window-ſhutter,)—and by their motion diſcovered the violent motions by which the ſpirits of wine in the tube of the thermometer was agitated.

This tube, which is $\frac{43}{100}$ of an inch in diameter internally, and very thin, is compoſed of very tranſparent, colourleſs glaſs, which rendered the appearance clear and diſtinct, and exceedingly beautiful. On examining the motion of the ſpirits of wine with a lens, I found that the aſcending current occupied the *axis of the tube*, and that it deſcended by the *ſides of the tube*.

On inclining the tube a little, the *riſing* current moved out of the axis, and occupied that ſide of the tube which was uppermoſt, while the *deſcending* current occupied the whole of the lower ſide of it.

When the cooling of the ſpirits of wine in the tube was haſtened, by wetting the tube with ice-cold water, the velocities of both the aſcending and the deſcending currents were ſenſibly accelerated.

The velocity of theſe currents was gradually leſſened, as the thermometer was cooled; and when it had acquired nearly the temperature of the air of the room, the motion ceaſed entirely.

By wrapping up the bulb of the thermometer in furs, or any other warm covering, the motion might be greatly prolonged.

I repeated the experiment with a ſimilar thermometer, of equal dimenſions, filled with linſeed-oil, and the appearances, on ſetting it in the window to cool, were juſt the ſame. The directions of the currents, and the parts they occupied in the tube, were the ſame; and their motions were, to all appearance, quite as rapid as thoſe in the thermometer which was filled with ſpirits of wine.

Having now no longer any doubt with reſpect to the *cauſe* of theſe appearances, being perſuaded that the motion in theſe liquids was occaſioned by their particles *going individually*, and *in ſucceſſion*, to give off their Heat to the cold ſides of the tube, in the ſame manner as I have ſhown in another place, that the particles of air give off *their* Heat to other bodies, I was led to conclude that theſe, and probably all other liquids, are in fact *non-conductors* of Heat; and I went to work immediately to contrive experiments to put the matter out of all doubt.

On conſidering the ſubject attentively, it appeared to me, that if liquids were in fact *non-conductors* of Heat, or if it be propagated in them *only* in conſequence of the internal motions of their particles; in that caſe, every thing which tends to obſtruct thoſe motions, ought certainly to retard the operation, and render the propagation of the heat ſlower, and more difficult. I had found that this is actually the caſe in reſpect to air; and though (under the influence of a ſtrong and deep-rooted prejudice) I had, from the reſult of one imperfect experiment, too haſtily concluded that it did not take place in regard to water; yet I now found ſtrong reaſons to call in queſtion the reſult of that experiment, and to give the ſubject a careful and thorough inveſtigation.

Thinking that the beſt mode of proceeding, in this inquiry, would be to adopt a method ſimilar to that I had purſued in my experiments on the conducting power of Air, I prepared an apparatus ſuitable to that purpoſe. The firſt object I had in view being to diſcover whether the propagation of Heat through water was obſtructed or not, by rendering the internal motion among the particles of the water, occaſioned by their change of temperature, embarraſſed and difficult, I contrived to make a certain quantity of Heat paſs through a certain quantity of pure water, confined in a certain ſpace; and noting the time employed in this operation, I repeated the experiment again, with the ſame apparatus, with this difference only, that in this ſecond trial, the water through which the Heat was made to paſs, inſtead of being *pure*, was mixed with a ſmall quantity of ſome fine ſubſtance, (ſuch as *eider-down*, for inſtance,)—which, without altering any of its chemical properties, or impairing its fluidity, ſerved merely to obſtruct and embarraſs the motions of the particles of the water in tranſporting the Heat, in caſe Heat ſhould be actually *tranſported* or *carried* in this manner, and not ſuffered to paſs freely through that liquid.

The body which received the Heat, and which ſerved, at the ſame time, to meaſure the quantity of it communicated, was a very large cylindrical thermometer. (See Plate I.) The bulb of this thermometer, which is conſtructed of thin ſheet-copper, is cylindrical, its two ends being hemiſpheres.

Its dimenſions are as follows:

Dimenſions of the bulb of the thermometer.

Diameter	- -	1.84 inches.
Length	- -	4.99 ——
Capacity or contents		13.2099 cubic inches.
External ſuperficies		28.834 ſuperficial inches.

The thickneſs of the ſheet-copper of which it is conſtructed, is 0.03 of an inch. It weighs, empty, 1846 grains;—and is capable of containing 3344 grains of water, at the temperature of 55°. This copper bulb has a glaſs tube, 24 inches long, and $\frac{4}{10}$ of an inch in diameter; which is fitted by means of a good cork, into a cylindrical tube or neck of copper, one inch long, and $\frac{65}{100}$ of an inch in diameter, belonging to the metallic bulb.

This thermometer, being filled with linſeed-oil, and its ſcale graduated, was fixed in the axis of a hollow cylinder, conſtructed of thin ſheet-copper, $11\frac{1}{2}$ inches long; and 2.3435 inches in diameter internally. This cylinder, which is open at one end, is cloſed at the other with a hemiſpherical bottom, with its convex ſurface outwards. The cylinder weighs 2261 grains, and the ſheet-braſs, of which it is conſtructed, is 0.0128 of an inch in thickneſs.

The bulb of the thermometer was placed in the lower part of this braſs cylindrical tube, and was confined in the middle, or axis of it, by means of three pins of wood, about $\frac{1}{10}$ of an inch in diameter, and $\frac{1}{4}$ of an inch long, which pins are fixed in tubes of thin ſheet-braſs $\frac{1}{10}$ of an inch in diameter, and $\frac{3}{20}$ of an inch in length. Theſe ſhort tubes, which are placed at proper diſtances on the inſide of the large braſs tube, at its lower end, and firmly attached to it by ſolder, ſerve as ſockets into which the ends of the wooden pins are fixed, which, pointing inwards, or towards the axis of the large cylindrical tube, ſerve to confine the lower end of the bulb of the thermometer in its proper place. Its upper end is kept in its place; or the axis of the thermometer is made to coincide with the axis of the braſs cylinder, by cauſing the tube of the thermometer to paſs through a hole in the middle of a cork ſtopper which cloſes the end of the cylinder.

The bottom of the bulb of the thermometer does not repoſe on the hemiſpherical bottom of the braſs cylinder, but is ſupported at the diſtance of ¼ of an inch above it, on the end of a wooden pin, like thoſe juſt deſcribed; which pin is fixed in a ſocket in the middle of the bottom of the cylindrical tube, and projects upwards. The ends of all theſe wooden pins, which project beyond the ſockets in which they are fixed, are reduced to a blunt point: This was done to reduce as much as poſſible the points of contact between the ends of theſe pins and the bulb of the thermometer.

The thermometer being in its place, there is on every ſide a void ſpace left between the bulb of the thermometer and the internal ſurface of the braſs cylinder in which it is confined; the diſtance between the external ſurface of the bulb of the thermometer and the internal ſurface of the containing cylinder being 0.25175 of an inch. This ſpace is deſigned to contain the water and other ſubſtance through which the Heat is made to paſs *into*, or *out* of the bulb of the thermometer; and the quantity of Heat which has paſſed, is ſhown by the height of the fluid in the tube of the thermometer.

The quantity of water required to fill this ſpace and to cover the upper end of the bulb of the thermometer to the height of about $\frac{1}{4}$ of an inch was found to weigh 2468 grains. As the thermometer was plunged into this water it was of courſe in contact with it by its whole ſurface, which, as we have ſeen, is equal to 28.834 ſquare inches.

The bulb of the thermometer being ſurrounded by water, or by any other liquid, or mixture, the conducting power of which was to be aſcertained, a cylinder of cork ſomething leſs in diameter than the braſs cylinder,—about half an inch long, with a hole in its centre, in which the tube of the thermometer paſſed freely,—was thruſt down into the braſs cylinder, but not quite ſo low as to touch the ſurface of the water, or other ſubſtance it contained. This cylinder, or diſk, was ſupported in its proper place by three projecting braſs points or pins, which were fixed with ſolder to the outſide of the metallic neck of the bulb of the thermometer.

As ſoon as this diſk of cork is put into its place, the upper part of the hollow braſs cylindrical tube is filled with eider-down, and it is cloſed above with its cork ſtopper, the tube of the thermometer, which paſſes through a fit hole in the middle of this ſtopper, projecting upwards. As the whole ſcale of the thermometer, from the point of freezing, to that of boiling water, is above the upper ſurface of this ſtopper, all the changes of heat to which the inſtrument is expoſed, can be obſerved at all times without deranging any part of the apparatus.

The thermometer is divided according to the ſcale of Fahrenheit, and its diviſions are made to correſpond with a very accurate mercurial thermometer made by TROUGHTON.

The experiments with this inſtrument, which, for the ſake of diſtinction, I ſhall call my *cylindrical paſſage thermometer*, were made in the following manner: The thermometer being fixed in its cylindrical braſs tube, in the manner above deſcribed, and ſurrounded by the ſubſtance the conducting power of which was to be aſcertained, the inſtrument was placed in thawing ice, where it was ſuffered to remain till the thermometer fell to 32°. It was then taken out of the melting ice, and immediately plunged into a large veſſel of boiling water, and the conducting power of the ſubſtance which was the ſubject of the experiment was eſtimated by the time employed by the Heat in paſſing through it into the thermometer; the time being carefully noted when the liquid in the thermometer arrived at the 40th degree of its ſcale; and alſo when it came to every 20th degree above it.

As the ſlower Heat moves, or is tranſported, in any medium, the longer muſt of courſe be the time required for any given quantity of it to paſs through it; and as the thermometer ſhows the changes which take place in the temperature of the body which is heated or cooled,—(namely, the liquid with which the thermometer is filled,—in conſequence of the paſſage of the Heat through the medium by which the thermometer is ſurrounded, the conducting power of that medium is ſhown by the quickneſs of the aſcent or deſcent of the thermometer, when, having been previouſly brought to a certain temperature, the inſtrument is ſuddenly removed and plunged into another medium at any other conſtant given temperature.

Having ſtill freſh in my memory the accidents I had ſo often met with in eating hot Apple-pies, I was very impatient, when I had completed this inſtrument, to ſee if Apples, which, as I well knew, are compoſed almoſt entirely of water, really poſſeſs a greater power of retaining Heat than that liquid when it is pure, or unmixed with other bodies. But before I made the Experiment, in order that its reſult might be the more ſatisfactory, I determined, in the following manner, how much water there really is in Apples, and what proportion their fibrous parts bear to their whole volume.

960 Grains of ſtewed Apples (the Apples having been carefully pared and freed from their ſtems and ſeeds before they were ſtewed) were well waſhed in a large quantity of cold ſpring water, and the fibrous parts of the Apples being ſuffered to ſubſide to the bottom of the veſſel, the clear part of the liquor was poured off, and the fibrous remainder being thoroughly dried was carefully weighed, and was found to weigh juſt 25 grains.

This fibrous remainder of the 960 grains of ſtewed Apples being again waſhed in a freſh quantity of cold ſpring water, and afterwards very thoroughly dried, by being expoſed ſeveral days on a china plate placed on the top of a German ſtove, which was kept conſtantly hot, was again weighed, and was found to weigh no more than $18\frac{9}{10}$ grains.

From this Experiment it appears that the fibrous parts of ſtewed Apples amount to leſs than $\frac{1}{50}$ part of the whole maſs; and there is abundant reaſon to conclude that the remainder, amounting to $\frac{49}{50}$ of the whole, is little elſe than pure water.

Having ſurrounded the bulb of my cylindrical paſſage thermometer with a quantity of theſe ſtewed Apples, (the conſiſtence of the maſs being ſuch that it ſhewed no ſigns of fluidity,) the inſtrument was placed in pounded ice, which was melting, and when the thermometer indicated that the whole was cooled down to the temperature of 32°, the inſtrument was taken out of the melting ice and plunged into a large veſſel of boiling water, and the water being kept boiling with the utmoſt violence during the whole time the Experiment laſted, the times taken up in heating the thermometer from 20 to 20 degrees were obſerved and noted down in a table, which had been previouſly prepared for that purpoſe.

This Experiment having been repeated twice, and varied as often by firſt heating the inſtrument to the temperature of boiling water, and then plunging it into melting ice, and obſerving the time taken up in the paſſage of the heat *out* of the thermometer; I removed the ſtewed Apples which ſurrounded the bulb of the thermometer, and filling the ſpace they had occupied with *pure water*, I now repeated the Experiments again with that liquid. The following Tables ſhow the reſults of theſe Experiments.

	Time the Heat was paſſing INTO the Thermometer,			
	Through ſtewed Apples.		Through Water.	
	Exp. No. 1.	Exp. No. 3.	Exp. No. 5.	Exp. No. 7.
	Seconds.	Seconds.	Seconds.	Seconds.
In heating the Thermometer from the temperature of 32° to that of - 40°	95	89	45	45
from 40° to 60°	75	67	36	35
60° to 80°	61	56	34	31
100°	65	60	30	30
120°	73	66	37	36
140°	90	82	44	44
160°	121	113	63	60
180°	188	170	93	90
200°	360	364	226	215
Total times in heating from 32° to 200°	1128	1057	608	586
Times employed in heating the inſtrument 80 degrees, viz. from 80° to 160°	349″	321″	174″	170″
Mean times in heating it from 80° to 160°	In ſtewed Apples 335″		In Water 172″	

The reſults of theſe Experiments ſhow that Heat paſſes with much greater difficulty, or much ſlower, in *ſtewed Apples* than in *pure Water*; and as ſtewed Apples are little elſe than water mixed with a very ſmall proportion of fibrous and mucilaginous matter, this ſhows that the conducting power of water with regard to Heat *may be impaired*.

The

The reſults of the following Experiments will ſerve to confirm this concluſion.

	Time the Heat was paſſing OUT OF the Thermometer,			
	Through ſtewed Apples.		Through Water.	
	Exp. No. 2.	Exp. No. 4.	Exp. No. 4.	Exp. No. 8.
	Seconds.	Seconds	Seconds.	Seconds.
In cooling the Thermometer from the temperature of 200° to that of 180°	80	74	46	37
from 180° to 160°	75	72	42	37
160° to 140°	84	83	43	43
120°	107	101	54	51
100°	141	136	73	73
80°	198	190	112	105
60°	321	307	200	204
40°	775	733	483	461
Total time in cooling from 200° to 40°	1781	1696	1053	1011
Times employed in cooling the inſtrument 80 degrees, viz. from 160° to 80°	530″	510″	282″	272″
Mean time in cooling it from 160° to 80°	In ſtewed Apples 520″		In Water 277″	

As the heating or cooling of the inſtrument goes on very ſlowly, when it approaches to the temperature of the medium in which it is placed, while, on the other hand, this proceſs is very rapid when, the temperature of the inſtrument being very different from that of the medium, it is firſt plunged into it, both theſe circumſtances conſpire to render the obſervations made at the extremities of the ſcale of the thermometer more ſubject to error, and conſequently leſs ſatisfactory than thoſe made nearer the middle of it: In order that the general concluſions drawn from the reſult of the Experiments might not be vitiated by the effects produced by theſe unavoidable inaccuracies, inſtead of eſtimating the celerity of the paſſage of the Heat by the times elapſed in heating and cooling the thermometer *through the whole length of its ſcale*, or between the point of freezing to that of boiling water, I have taken the times elapſed in heating and cooling it 80 *degrees in the middle of the ſcale*, viz. between 80° and 160°, as the meaſure of the conducting powers of the ſubſtances through which the Heat was made to paſs.

I have, however, noted the times which elapſed in heating and cooling the inſtrument through a much larger interval, namely, through an interval of 168 degrees in *heating*,—or from 32° to 200°,—and in *cooling* through 160 degrees, or from 200° to 40°.

In reſpect to the *cooling* of the inſtrument, it is neceſſary that I ſhould inform my reader, that though I have not in the Tables of the Experiments mentioned any higher temperature than that of 200°, yet the inſtrument was always heated to the point of boiling water, which, under the preſſure of the atmoſphere at Munich, where the Experiments were made, was commonly about 209½ deg. of Fahrenheit's ſcale. The inſtrument being kept in boiling water till its thermometer appeared to be quite ſtationary, was then taken out of the water, and inſtantly plunged into melting ice, and the time was obſerved and carefully noted down when the liquid in its thermometer paſſed the diviſion of its ſcale which indicated 200°, as alſo when it arrived at the other diviſions indicated in the Tables.

With regard to the four laſt mentioned Experiments, (No. 2, 4, 6, and 8,) it will be found on examination that their reſults correſpond very exactly with thoſe before deſcribed; and they certainly prove in a very deciſive manner this important fact:—*that a ſmall proportion of certain ſubſtances, on being mixed with water, tend very powerfully to impair the conducting power of that Fluid in regard to Heat.*

In the Experiments No. 1 and No. 2, which were both made on the ſame day, and in the order in which they are numbered, the Heat was conſiderably more obſtructed in its paſſage through the maſs of ſtewed Apples which ſurrounded the thermometer than in the Experiments No. 3 and No. 4, which were made on the following day. It is probable that this was occaſioned by ſome change in the conſiſtency of this ſoft maſs of the ſtewed Apples which had taken place while the inſtrument was left to repoſe in the interval between the Experiments: but, inſtead of ſtopping to ſhow how this might be explained, I ſhall proceed to give an account of ſome Experiments from the reſults of which we ſhall derive information that will be much more ſatisfactory than any ſpeculations I could offer on that ſubject.

Suppoſing Heat to be propagated in water *in the ſame manner* as it is propagated in air and other elaſtic Fluids, namely, that it is *tranſported* by its particles, theſe particles being put in motion by the change which is produced in their ſpecific gravity, by the change of temperature, and that there is no communication whatever, or *interchange of their Heat*, among the particles of *the ſame Fluid*,—in that caſe, it is evident that the Propagation of Heat in a Fluid may be conſtructed in two ways, namely, by diminiſhing its *Fluidity*, (which may be done by diſſolving in it any mucilaginous ſubſtance,) or, more ſimply, by merely embarraſſing and obſtructing the motion of its particles in the operation of *tranſporting* the Heat, which may be effected by mixing with the Fluid any ſolid ſubſtance,—(it muſt be a non-conductor of Heat however,)—in ſmall maſſes, or which has a very large ſurface in proportion to its ſolidity.

In the foregoing Experiments with *ſtewed Apples*, the paſſage of the Heat in the water, (which conſtituted by far the greateſt part of the maſs,) was doubtleſs obſtructed in both theſe ways. The mucilaginous parts of the Apples diminiſhed very much the fluidity of the water, at the ſame time that the fibrous parts ſerved to embarraſs its internal motions.

In order to diſcover the *comparative effects* of theſe two cauſes, it was neceſſary to ſeparate them; or to contrive Experiments, in which only one of them ſhould be permitted to act at the ſame time. This I endeavoured to do in the following manner.

To aſcertain the effects produced by diminiſhing the *fluidity* of water, I mixed with it a ſmall quantity of *ſtarch*, namely, 192 grains in weight to 2276 grains of water;—and to determine the effects produced by merely *embarraſſing* the water in its motions, I mixed with it an equal proportion (by weight) of EIDER-DOWN. The ſtarch was boiled with the water with which it was mixed; as was alſo the eider-down. This laſt mentioned ſubſtance was boiled in the water in order to free it from air, which, as is well known, adheres to it with great obſtinacy.

In order that theſe Experiments might, with greater facility, be compared with thoſe which were made with *ſtewed Apples*, and with *pure Water*, I ſhall place their reſults all together, in the following Tables.

	Time the Heat was in paſſing INTO the Thermometer,			
	Through a Mixture of 2276 grains of Water, and 192 grs. of STARCH.	Through a Mixture of 2276 grains of Water, and 192 grs. of EIDER-DOWN.	Through STEWED APPLES.	Through pure WATER.
	Experiment No. 9.	Experiment No. 11.	Mean of two Exp. N° 1 & N° 3.	Mean of two Exp. N° 5 & N° 7.
	Seconds.	Seconds.	Seconds.	Seconds.
In heating the Thermometer from 32° to 40°	101	83	92	45
from 40° to 60°	72	55	71	35½
60° to 80°	64	49	58½	32½
100°	63	52	62½	30
120°	74	57	69½	36½
140°	89	67	86	44
160°	115	93	117	61½
180°	178	133	179	91½
200°	453	360	362	220½
Total times in heating the inſtrumt from 32° to 200°	1109	949	1096½	597
Times employed in heating the Thermometer 80 degrees, viz. from 80° to 160°	341″	269″	335″	172″

	Time the Heat was in paſſing OUT OF the Thermometer,			
	Through a Mixture of 2276 grains of Water, & 192 grs. of STARCH.	Through a Mixture of 2276 grains of Water, and 192 grs. of EIDER-DOWN.	Through STEWED APPLES.	Through pure WATER.
	Exp. No. 10.	Experiment No. 12.	Mean of two Exp. N°2 & N°4.	Mean of two Exp. N°6 & N°8.
	Seconds.	Seconds.	Seconds.	Seconds.
In cooling the Thermometer from 200° to 180°	69	68	77	41½
from 180 to 160°	66	61	73½	39½
160° to 140°	74	72	83½	43
120°	92	91	104	52½
100°	119	120	138½	73
80°	173	177	194	108½
60°	283	279	314	202
40°	672	673	754	472
Total times in cooling from 200° to 40°	1548	1541	1749½	1032
Times employed in cooling the inſtrument 80 degrees, viz. from 160° to 80°	468″	460″	520″	277″

As the reſults of theſe Experiments prove, in the moſt deciſive manner, that the Propagation of Heat in water is retarded, not only by thoſe things which diminiſh its fluidity, but alſo by thoſe which, by mechanical means, and without forming any combination with it whatever, merely obſtruct its internal motions; it appears to me, that this proves, almoſt to a demonſtration, that Heat is propagated in water *in conſequence* of its internal motions;—or that it is tranſported or *carried* by the particles of that liquid, and that it does not ſpread and expand in it as has generally been imagined.

I have ſhewn in another place, and I believe I may venture to ſay I have proved*, that Heat is actually propagated in *air*, in the ſame manner I here ſuppoſe it to be propagated in water; and if the conducting powers of both theſe fluids are found to be impaired by the *ſame means*, it affords very ſtrong grounds to conclude that they both conduct Heat in the *ſame manner:* but this has been found to be actually the caſe.

Eider-down, which cannot affect the ſpecifical qualities of either of thoſe fluids, and which certainly does no more when mixed with them, than merely to obſtruct and embarraſs their internal motions, has been found to retard very much the Propagation of Heat in both of them: on comparing theſe Experiments with thoſe I formerly made on the conducting power of air, it will even be found, that the conducting power of water is nearly, if not quite, as much impaired by a mixture of *eider-down* as that of air.

In the courſe of my Experiments on the various ſubſtances uſed in forming artificial clothing for confining Heat, I found that the thickneſs of a ſtratum of air, which ſerved as a barrier to Heat, remaining the ſame, the paſſage of Heat through it was ſometimes rendered more difficult by increaſing the quantity of the light ſubſtance which was mixed with it to obſtruct its internal motion.

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* See Philoſophical Tranſactions, 1792.

To ſee if ſimilar effects would be produced by the ſame means when Heat is made to paſs through water, I repeated the Experiments with *eider-down*, reducing the quantity of it mixed with the water to 48 grains, or *one quarter* of the quantity uſed in the Experiments No. 11 and No. 12.

The reſults of theſe Experiments, and a compariſon of them with thoſe before mentioned, may be ſeen in the following Tables:

	Time the Heat was in paſſing INTO the Thermometer,		
	Through Water with 48 grs. or $\frac{1}{50}$ of its bulk of EIDER-DOWN.	Through Water with 192 grs. or $\frac{4}{50}$ of its bulk of EIDER-DOWN.	Through pure WATER.
	Experiment No. 13.	Experiment No. 11.	Mean of two Exp. N° 5 & N° 7.
	Seconds.	Seconds.	Seconds.
In heating the Thermometer from 32° to 40°	51	83	45
from 40° to 60°	47	55	35½
60° to 80°	39	49	32½
100°	40	52	30
120°	45	57	36½
140°	56	67	44
160°	74	93	61½
180°	118	133	91½
200°	293	360	220½
Total times in heating from 32° to 200°	763	949	597
Times employed in heating the inſtrument 80 degrees, or from 80° to 160°	215″	269″	172″

	Time the Heat was paſſing OUT OF the Thermometer.		
	Through Water with 48 grs. or $\frac{1}{50}$ of its bulk of EIDER-DOWN.	Through Water with 192 grs. or $\frac{4}{50}$ of its bulk of EIDER-DOWN.	Through pure WATER.
	Experiment No. 14.	Experiment No. 12.	Mean of two Exp. N°6 & N° 8.
	Seconds.	Seconds.	Seconds.
In cooling the Thermometer from 200° to 180°	49	68	41½
from 180° to 160°	50	61	39½
160° to 140°	56	72	43
120°	70	91	52½
100°	96	120	73
80°	151	177	108½
60°	262	279	202
40°	661	673	472
Total times in cooling from 200° to 40°	1395	1541	1032
Times employed in cooling the inſtrument 80 degrees, viz. from 160° to 80°	373″	460″	277″

The reſults of theſe Experiments are extremely intereſting. They not only make us acquainted with a new and very curious fact,—namely, that feathers, and other like ſubſtances, which, *in air*, are known to form very warm covering for confining Heat, not only ſerve the ſame purpoſe *in water*, but that their effects in preventing the paſſage of Heat is even greater in water than in air.

This diſcovery, if I do not deceive myſelf, throws a very broad light over ſome of the moſt intereſting parts of the economy of Nature, and give us much ſatisfactory information reſpecting the final cauſes of many phænomena which have hitherto been little underſtood.

As *liquid water* is the vehicle of Heat and nouriſhment, and conſequently of *life*, in every living thing; and as water, left to itſelf, freezes with a degree of cold much leſs than that which frequently prevails in cold climates, it is agreeable to the ideas we have of the wiſdom of the Creator of the world, to expect that effectual meaſures would be taken to preſerve a ſufficient quantity of that liquid in its fluid ſtate, to maintain life during the cold ſeaſon: and this we find has actually been done; for both plants and animals are found to ſurvive the longeſt and moſt ſevere winters; but the means which have been employed to produce this miraculous effect have not been inveſtigated;—at leaſt not, in as far as they relate to vegetables.

But as animal and vegetable bodies are eſſentially different in many reſpects, it is very natural to ſuppoſe, that the means would be different which are employed to preſerve them againſt the fatal effects which would be produced in each by the congelation of their fluids.

Among organized bodies, which live on the ſurface of the earth, and which of courſe are expoſed to the viciſſitudes of the ſeaſons, we find that as the proportion of fluids to ſolids is greater, the greater is the Heat which is required for the ſupport of life and health; and the leſs are they able to endure any conſiderable change of their temperature.

The proportion of Fluids to Solids is much greater in *animals* than in vegetables; and in order to preſerve in them the great quantity of Heat which is neceſſary to the preſervation of life, they are furniſhed with lungs, and are warmed by a proceſs ſimilar to that by which Heat is generated in the combuſtion of inflammable bodies.

Among *vegetables*, thoſe which are the moſt ſucculent are *annual*. Not being furniſhed with lungs to keep the great maſs of liquids warm, which fill their large and ſlender veſſels, they live only while the genial influence of the ſun warms them, and animates their feeble powers; and they droop and die as ſoon as they are deprived of his ſupport.

There are many tender plants to be found in cold countries, which die in the autumn, the roots of which remain alive during the winter, and ſend off freſh ſhoots in the enſuing ſpring. In theſe we ſhall conſtantly find the roots more compact and denſe than the ſtalk, or with ſmaller veſſels, and a ſmaller proportion of Fluids.

Among the trees of the foreſt we ſhall conſtantly find, that thoſe which contain a great proportion of *thin watery liquids*, not only ſhed their leaves every autumn, but are ſometimes frozen, and actually killed, in ſevere froſts. Many thouſands of the largeſt walnut-trees were killed by the froſt in the Palatinate, during the very cold winter in the year 1788; and it is well known that few, if any, of the deciduous plants of our temperate climate would be able to ſupport the exceſſive cold of the frigid zone.

The trees which grow in thoſe inhoſpitable climates, and which brave the colds of the ſevereſt winters, contain very little watery liquids. The ſap which circulates in their veſſels is thick and viſcous, and can hardly be ſaid to be fluid. Is there not the ſtrongeſt reaſon to think, that this was ſo contrived for the expreſs purpoſe of preventing their being deprived of all their Heat, and killed by the cold during the winter?

We have ſeen by the foregoing Experiments, how much the Propagation of Heat in a liquid is retarded by diminiſhing its fluidity; and who knows but this may continue to be the caſe, as long as any degree of fluidity remains?

As the bodies and branches of trees are not covered in winter by the ſnow which protects their roots from the cold atmoſphere, it is evident that extraordinary meaſures were neceſſary to prevent their being frozen. The bark of all ſuch trees as are deſigned by nature to ſupport great degrees of cold, forms a very warm covering; but this precaution alone would certainly not have been ſufficient for their protection. The ſap, in all trees which are capable of ſupporting a long continuance of froſt, grows thick and viſcous on the approach of winter. What more important purpoſe could this change anſwer, than that here indicated?—

And it would be more than folly to pretend that it anſwers no uſeful purpoſe at all?

We have ſeen, by the reſults of the foregoing Experiments, how much the ſimple embarraſſment of liquids in their internal motions tends to retard the Propagation of Heat in them, and conſequently its paſſage out of them;—and when we conſider the extreme ſmallneſs of the veſſels in which the ſap moves in vegetables, and particularly in large trees;—when we recollect that the ſubſtance of which theſe ſmall tubes are formed, is one of the beſt non-conductors of Heat known*;—and when we advert to the additional embarraſſments to the paſſage of the Heat, which ariſe from the increaſed viſcoſity of the ſap in winter, and to the almoſt impenetrable covering for confining Heat, which is formed by the bark, we ſhall no longer be at a loſs to account for the preſervation of trees during the winter, notwithſtanding the long continuation of the hard froſts to which they are annually expoſed.

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* I lately, by accident, had occaſion to obſerve a very ſtriking proof of the extreme difficulty with which Heat paſſes in wood. Being preſent at the foundry at Munich, when cannons were caſting, I obſerved that the founder uſed a wooden inſtrument for ſtirring the melted metal. It was a piece of oak plank, green, or unſeaſoned, about ten inches ſquare and two inches thick, with a long wooden handle, which was fitted into a hole in the middle of it. As this inſtrument was frequently uſed, and ſometimes remained a conſiderable time in the furnace, in which the Heat was moſt intenſe, I was ſurpriſed to find that it was not conſumed; but I was ſtill more ſurpriſed, on examining the part of the plank which had been immerſed in the melted metal, to find that the Heat had penetrated it to ſo inconſiderable a depth, that, at the diſtance of one twentieth of an inch below its ſurface, the wood did not ſeem to have been in the leaſt affected by it. The colour of the wood remained unchanged, and it did not appear to have loſt even its moiſture.

On the ſame principles we may, I think, account, in a ſatisfactory manner, for the preſervation of ſeveral kinds of fruit,—ſuch as apples and pears for inſtance,—which are known to ſupport, without freezing, a degree of cold, which would ſoon reduce an equal volume of *pure water* to a ſolid maſs of ice.

At the ſame time that the compact ſkin of the fruit effectually prevents the evaporation of its fluid parts, which, as is well known, could not take place without occaſioning a very great loſs of Heat, the internal motions of thoſe fluids are ſo much obſtructed by the thin partitions of the innumerable ſmall cells in which they are confined, that the communication of their Heat to the air ought, according to our hypotheſis, to be extremely ſlow and difficult. Theſe fruits do, however, freeze at laſt, when the cold is very intenſe; but it muſt be remembered, that they are compoſed almoſt entirely of liquids, and of ſuch liquids as do not grow viſcous with cold; and moreover, that they were evidently not deſigned to ſupport, for a long time, very ſevere froſts.

Parſnips and carrots, and ſeveral other kinds of roots, ſupport cold without freezing, ſtill longer than apples and pears, but theſe are leſs watery; and I believe the veſſels in which their fluids are contained, are ſmaller; and both theſe circumſtances ought, according to our aſſumed principles, to render the paſſage of their Heat out of them more difficult, and conſequently to retard their congelation.

But there is ſtill another circumſtance, and a very remarkable one indeed, which, if our conjectures reſpecting the manner in which Heat is propagated in liquids be true, muſt act a moſt important part in the preſervation of Heat, and conſequently of animal and vegetable life, in cold climates. But as the probability of all theſe deductions muſt depend, very much, on the evidence which is brought to prove the great fundamental fact on which they are eſtabliſhed;—that reſpecting the internal motions among the particles of liquids, which *neceſſarily* take place when they are heated or cooled;—before I proceed any farther in theſe ſpeculations, I ſhall endeavour to throw ſome more light on that curious and intereſting ſubject.

CHAP. II.

Farther inveſtigations of the internal Motions among the Particles of Liquids which neceſſarily take place when they are heated or cooled.—Deſcription of a mechanical Contrivance by which theſe Motions in Water were rendered viſible.—An Account of various amuſing Experiments, which were made with this new-invented Inſtrument.—They lead to an important Diſcovery.—Heat cannot be propagated DOWNWARDS *in Liquids, as long as they continue to be condenſed by Cold.—Ice found, by Experiment, to melt more than* EIGHTY TIMES *ſlower, when boiling-hot Water ſtood on its Surface, than when the Ice was ſuffered to ſwim on the Surface of the hot Water.—The melting of Ice by Water ſtanding on its Surface can be accounted for, even on the Suppoſition that Water is a perfect Non-conductor of Heat.—According to the aſſumed Hypotheſis, Water only eight Degrees of Fahrenheit's Scale above the freezing Point, or at the Temperature of 40°, ought to melt as much Ice, in any given Time, when ſtanding on its Surface, as an equal Volume of that Fluid, at any higher Temperature, even were it boiling-hot.—This remarkable Fact is proved by a great Variety of deciſive Experiments.—Water at the Temperature of 41° is found to melt even* MORE *Ice, when ſtanding on its Surface, than boiling-hot Water.—The Reſults of all*

all theſe Experiments tend to prove that Water is, in fact, a perfect Non-conductor of Heat; or that Heat is propagated in it, merely in conſequence of the Motions it occaſions among the inſulated or ſolitary Particles of that Fluid, which, among themſelves, have no Communication or Intercourſe whatever in this Operation.—The Diſcovery of this Fact opens to our View one of the grandeſt and moſt intereſting Scenes in the Economy of Nature.

As the particles of water, as alſo of all other Fluids, are infinitely too ſmall to be ſeen by human eyes, their motions muſt of courſe be imperceptible by us; but we are frequently enabled to judge, with the utmoſt certainty, of the motions of inviſible Fluids, by the motions they occaſion in viſible bodies.

Air is an inviſible Fluid, but we acquire very juſt notions of the motions in air, by the duſt, and other light bodies which are carried along with it in its motions. Nobody who has ever ſeen a whirlwind ſweep over the ſurface of a ploughed field in dry weather, can have any doubt reſpecting the nature of the motions into which the air is thrown on thoſe occaſions; notwithſtanding that they are extremely complicated, and would be very difficult to deſcribe.

It was by the motions of the very fine particles of duſt, which by accident had been mixed with the ſpirits of wine in my large thermometer, and which, when ſtrongly illuminated by the direct beams of the ſun, became viſible, that I firſt diſcovered the internal motions in that Fluid, which take place when it is cooling; and, availing myſelf of this kind hint, I contrived to render the internal motions of water equally viſible. This, I immediately ſaw, could be done with the utmoſt facility, if I could but find any ſolid body *of the ſame ſpecific gravity as water*, which would be proper to mix with it;—that is to ſay, that would not be liable to be diſſolved by it, or to be reduced to ſuch ſmall particles as to become itſelf inviſible; but ſuch a ſubſtance was not to be found. On reflection it occurred to me, that it is very fortunate that ſuch ſubſtances do not abound; for otherwiſe we ſhould find great difficulty in procuring water in a pure ſtate.

Not being able to find any ſolid ſubſtance fit for my purpoſe, of the ſame ſpecific gravity as pure water, I was obliged to have recourſe to the following ſtratagem.

Looking over the tables of ſpecific gravities, I found that the ſpecific gravity of tranſparent *yellow amber* was but a little greater than that of water, being 1.078, while that of water is 1.000; and it occurred to me, that by diſſolving in the water a certain quantity of pure alkaline ſalt, I might augment its ſpecific gravity, or rather bring the ſpecific gravity of the ſolution to be preciſely equal to that of the amber, without impairing the tranſparency of the liquid, or changing any of its properties by which the manner of its receiving and tranſporting Heat could be ſenſibly affected.

This contrivance was put in execution in the following manner, with complete ſucceſs. Having provided myſelf with a number of glaſs globes of various ſizes, with long cylindrical necks, I choſe one which was about two inches in diameter, with a cylindrical neck ¾ of an inch in diameter, and twelve inches long; and putting into it about half a tea-ſpoonful of yellow amber, in the form of a coarſe powder, (the pieces, which were irregular in their forms, and tranſparent, being about the ſize of muſtard-ſeeds,) I poured upon it a certain quantity of diſtilled water, which was at the temperature of the air in my room,—(about 60° F.)

Finding, as I expected, that the amber remained at the bottom of the globe, I now added to the water as much of a ſaturated ſolution of pure vegetable alkali, as was ſufficient to increaſe the ſpecific gravity of the water, (or rather of the diluted ſaline ſolution,) till the pieces of amber began to float, and remained apparently motionleſs, in any part of the liquid where they happened to reſt.

As the glaſs body was not yet as full as I wiſhed, I continued to add more of the alkaline ſolution, and of water, in due proportions, till the globe was full; and alſo till its cylindrical tube was filled to within about three inches of its end; and then cloſed it well with a clean cork.

Having ſhaken the contents of this glaſs body well together, I placed it, with its cylindrical tube in a vertical poſition, on a wooden ſtand, and left it to repoſe in quiet, in order to ſee how long the ſolid particles of amber—(which appeared to be very

equally

equally diſperſed about in the whole maſs of the liquid)—would remain ſuſpended.

Though the greater number of theſe particles ſeemed at firſt to have no tendency either to aſcend or to deſcend, yet ſome of them ſoon began to move very ſlowly upwards, and others to move as ſlowly downwards; and as theſe particles were moving at the ſame time promiſcuouſly in all parts of the ſame liquid, and even in the ſame part of it in both directions at the ſame time, (the aſcending and deſcending particles frequently paſſing each other ſo near as to touch,) I ſaw that theſe motions were independent of any internal motion of the liquid, and aroſe merely from the difference of the ſpecific gravity of the different ſmall pieces of the amber, and of that of the liquid. Some of the pieces of amber, being evidently heavier than the liquid, moved downward, while others, which were lighter, aſcended to its ſurface.

Finding that there was ſo much difference in the ſpecific gravities of the different pieces of amber, I now added more of this ſubſtance to the liquid, and ſuffering it to ſubſide after I had ſhaken it well together, I gently poured off what had riſen to the top of the liquid, and retaining only that which had ſettled at the bottom of it, I increaſed the ſpecific gravity of the liquid by adding a little of the alkaline ſolution, till the ſmall pieces of amber which remained in the glaſs were juſt buoyed up and ſuſpended in the different parts of the Fluid, where they now ſeemed to have taken their permanent ſtations.

I had now an inſtrument which appeared to me to be well calculated for the very intereſting Experiments I had projected, and it will eaſily be imagined that I loſt no time in making uſe of it.

The firſt Experiment I made with this inſtrument was to plunge it into a tall glaſs jar, nearly filled with water almoſt boiling hot. The reſult was juſt what I expected. Two currents, in oppoſite directions, began at the ſame inſtant to move with great celerity in the liquid in the cylindrical tube, the aſcending current occupying the ſides of the tube, while that which moved downwards occupied its axis.

As the ſaline liquor grew warm, the velocity of theſe currents gradually diminiſhed; and at length, when the liquor had acquired the temperature of the ſurrounding water in the jar, theſe motions ceaſed entirely.

On taking the glaſs body out of the hot water, the internal motions of the liquor recommenced; but the currents had changed their directions, that which occupied the axis of the tube being now the aſcending current.

When the cylindrical tube, inſtead of being held in a vertical poſition, was inclined a little, the aſcending current occupied that ſide of it which happened to be uppermoſt, while the under ſide of it was occupied by the current which moved (with equal velocity) downwards.

When the contents of the glaſs body had acquired the temperature of the air of the room, theſe motions ceaſed, but they immediately recommenced on expoſing the inſtrument to any change of temperature.

In all caſes where the inſtrument *received Heat*, the current in the axis of its cylindrical tube, when it was placed in a vertical poſition, (and that which occupied its *upper ſide* when it was inclined,) moved downwards.—When it parted with Heat its motion was in an oppoſite direction, that is to ſay, *upwards*.

A change of temperature amounting only to a few degrees of Fahrenheit's ſcale, was ſufficient to ſet the contents of the inſtrument in motion; and the motion was more or leſs rapid as the velocity was greater or leſs with which it acquired or parted with Heat, and the motion was moſt rapid in thoſe parts of the inſtrument where the communication of heat was not rapid.

A partial motion might at any time be produced in any part of the inſtrument, by applying to that part of it any body either hotter or colder than the inſtrument. If the body ſo applied were hotter than the inſtrument, the motion of the ſaline liquor in that part of it immediately in contact with the hot body, was *upwards*,—if colder, *downwards*; and whenever a hot or cold body produced a current upwards or downwards, this current immediately produced another in ſome other part of the liquid which flowed in an oppoſite direction.

On inclining the cylindrical tube of the inſtrument to an angle of about 45 degrees with the plane of the horizon, and holding the middle of it over the flame of a candle, at the diſtance of three or four inches above the point of the flame; the motion of the Fluid in the upper part of the tube became exceſſively rapid, while that in the lower end of it where it was united to the globe, as well as that in the globe itſelf, remained almoſt perfectly at reſt.

I even found that I could make the Fluid in the upper part of the tube *actually boil*, without that in the lower part of it appearing to the hand to be ſenſibly warmed. But when the flame was directed againſt the lower part of the tube, all the upper parts of it in contact with the liquid, and eſpecially that ſide of it which was uppermoſt as it lay in an inclined poſition, where the aſcending current was moſt rapid, and where it impinged againſt the glaſs, were very ſoon heated very hot.

The motions in oppoſite directions, in the liquid in the tube, were exceedingly rapid on this ſudden application of a ſtrong Heat, and afforded a very entertaining ſight:—but to a ſcientific obſerver they were much more than amuſing. They detected Nature, as it were, in the very act, in one of her moſt hidden operations, and rendered motions viſible in the midſt of an inviſible medium, which never had been obſerved before, and which moſt probably had never been ſuſpected.

Encouraged by this ſucceſs, and confirmed in my opinions reſpecting the intereſting fact I had undertaken to inveſtigate, I now proceeded with confidence to ſtill more direct and deciſive Experiments.

It is an opinion which, I believe, is generally received among philoſophers, that water cannot be heated in contact with ice: reflecting on the ſubject, I immediately perceived that either this muſt be a miſtake, or all my ideas reſpecting the manner in which Heat is propagated in that Fluid muſt be erroneous. I ſaw that as long as the ice floats at the ſurface of water which is attempted to be warmed over a fire, (or in any other way,) the ice-cold water which reſults from the melting of the ice, muſt, according to my own hypotheſis, deſcend, and ſpreading over the bottom of the containing veſſel, and, before it has time to be much heated, being in its turn forced to give place to the ice-cold water, which, as long as any ice remains, continues to deſcend in an uninterrupted ſtream as long as this operation is going on, the maſs of the water cannot be much heated; but on the ſuppoſition that water is not a conductor of Heat, according to the common acceptation of that term, or that Heat cannot paſs in that Fluid except when it is *carried* by its particles, which, being put in motion by the change it occaſions in their ſpecific gravity, *tranſports* it from place to place, it does not appear how ice, if inſtead of being permitted to ſwim on water, were confined at the bottom of it, or at any given diſtance below its ſurface, could, in any way affect the temperature of the ſuperincumbent water, or prevent its receiving Heat from other bodies.

Were water a conductor of Heat, there is no doubt but that the influence of the preſence of the ice would be propagated in the water in all directions.

The metals are all conductors of Heat, and Profeſſor Pictet found, by an ingenious and deciſive Experiment, that in a bar of copper, 33 inches in length, placed in a vertical poſition, Heat paſſed downwards, as well as upwards, and nearly with the ſame facility in both theſe directions* ; and if it can be ſhown that Heat cannot deſcend in water, that alone will, I imagine, be thought quite ſufficient to prove that water is not a conductor of Heat.

When we meditate profoundly on the nature of Fluidity, it ſeems to me that we can perceive ſome faint lights which might lead us to ſuſpect that the *cauſe*, and I may ſay the very *eſſence of fluidity*, is that property which the particles of bodies acquire when they become fluid, by which all farther interchange or communication of Heat among them is prevented. But however this may be, the reſult of the following Experiments will certainly be conſidered as affording indiſputable evidence of one important fact, reſpecting the manner in which Heat is propagated in water.

Experiment, No. 15.

Into a cylindrical glaſs jar 4.7 inches in diameter, and 14 inches high, I fitted a circular cake of ice nearly as large as the internal diameter of the jar, and 3½ inches thick, weighing 10⅛ oz.

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* Eſſais de Phyſique, tome 1. Genève 1790.

This cake of ice being ready, I now poured into the jar 6 lb. $1\frac{1}{4}$ oz. Troy, of boiling-hot water, and putting the ice gently into it, I found that it was entirely melted in 2 minutes and 58 ſeconds.

Having found by this Experiment how long the ice was in melting at the ſurface of the hot water, I now endeavoured to find out whether it would not require a longer time to melt at the bottom of the water.

Experiment, No. 16.

Into the ſame jar which was uſed in the foregoing Experiment, I now put a cake of ice of the ſame form and dimenſions as that above deſcribed, but inſtead of letting it ſwim at the ſurface of the hot water, I faſtened it down on the bottom of the jar, and poured the water upon it.

This cake of ice was faſtened down in the jar by means of two ſlender and elaſtic pieces of deal about $\frac{1}{8}$ of an inch thick, and $\frac{1}{4}$ of an inch wide, which, being a trifle longer than the internal diameter of the jar, were of courſe a little bent when they were introduced into it in an horizontal poſition, and on being put down upon the ice, at right angles to each other, ſerved to confine the ice, and prevent its riſing up to the ſurface when the water was put into the jar upon it.

To protect the ice while the boiling-hot water was pouring into the jar, its ſurface was covered with a circular piece of ſtrong writing paper, which was afterwards removed as gently as poſſible by means of a ſtring which was faſtened to one ſide of it; and to prevent the glaſs jar from being cracked by the ſudden application of the boiling-hot water, I began by pouring a ſmall quantity of cold water into the jar,—juſt enough to fill up the interſtices between the ice and the glaſs, and to cover the ice to the height of about $\frac{1}{4}$ of an inch; and in pouring the hot water into the jar, out of a large tea-kettle in which it had been boiled, I took care to direct the ſtream againſt the middle of the circular piece of paper which covered the ice.

The jar with the ice and the hot water in it being placed on a table near a window, I drew away, as gently as poſſible, the paper which covered the ſurface of the ice, and prepared myſelf to obſerve, at my eaſe, the reſult of this moſt intereſting Experiment.

A very few moments were ſufficient to ſhow me that my expectation with regard to it would not be diſappointed. In the former Experiment a ſimilar cake of ice had been entirely melted in leſs than three minutes; but in this, after more than twice that time had elaſped, the ice did not ſhow any apparent ſigns of even *beginning to melt.* Its ſurface remained ſmooth and ſhining, and the water immediately in contact with it appeared to be perfectly at reſt, through the internal motions of the hot water above it, which was giving off its heat to the ſides of the jar and to the air, were very rapid, as I could diſtinctly perceive by means of ſome earthy particles or other impurities which this water happened to contain.

I examined the ice with a very good lens, but it was a long time before I could perceive any ſigns of its melting. The edges of the cake remained ſharp, and the minute particles of duſt, which, by degrees, were precipitated by the hot water as it grew colder, remained motionleſs as ſoon as they touched the ſurface of the ice.

As the hot water had been brought from the kitchen in a tea-kettle, it was not quite boiling-hot when it was poured into the jar. After it had been in the jar one minute, I plunged a thermometer into it, and found its temperature to be at 180°.

After 12 minutes had elapſed, its temperature at the depth of one inch under the ſurface was 170°, At the depth of ſeven inches, or one inch above the ſurface of the ice, it was at $169\frac{1}{10}$; while at only $\frac{3}{4}$ of an inch lower, or $\frac{1}{4}$ above the ſurface of the ice, its temperature was 40°.

When 20 minutes had elapſed, the Heat in the water at different depths was found to be as follows:

Immediately above the ſurface of the ice	40°
At the diſtance of ½ an inch above it	46°
At 1 inch - - - -	130°
At 3 inches - - - -	159°
At 7 inches - - - -	160°

When 35 minutes had elapſed, the Heat was as follows:

At the ſurface of the ice - -	40°
½ an inch above it - - -	76°

1 inch

1 inch above it	110°
2 inches	144°
3 inches	148°
5 inches	148½°
7 inches	149°

At the end of one hour the Heat was as follows:

At the ſurface of the ice	40°
1 inch above it	80°
2 inches	118°
3 inches	128°
4 inches	130°
7 inches	131°

After 1 hour and 15 minutes had elapſed, the Heat was found to be as follows:

At the ſurface of the ice	40°
1 inch above it	82°
2 inches	106°
3 inches	123°

The Heat of the water had hitherto been taken near the ſide of the jar;—in the two following trials it was meaſured in the middle or axis of the jar.

When 1 hour and 30 minutes—(reckoning always from the time when the boiling-hot water was poured into the jar)—had elapſed, the Heat of the water in the middle of the jar was found to be as follows:

At the ſurface of the ice	40°
1 inch above it	84°
2 inches	115°
3 inches	116°
7 inches	117°

When 2 hours had elapſed, the Heat in the middle of the jar was found to be as follows:

At the ſurface of the ice	40°
1 inch above it	76°
2 inches	94°
3 inches	106°
4 inches	108°
6 inches	108¼°
7 inches	108½°

An end being now put to the Experiment, the hot water was poured off from the ice, and on weighing that which remained, it was found that 5 oz. 6 grains Troy (=2406 grains) of ice had been melted.

Taking the mean temperature of the water at the end of the Experiment at 106°, it appears that the maſs of hot water (which weighed 73¼ ounces) was cooled 78 degrees, or from the temperature of 184° to that of 106°, during the Experiment. Now, as it is known that one ounce of ice abſorbs juſt as much Heat in being changed to water as one ounce of water looſes in being cooled 140 degrees, it is evident that one ounce of water which is cooled 78 degrees, gives off as much Heat as would be ſufficient to melt $\frac{78}{140}$ of an ounce of ice.; conſequently the 73¼ ounces of hot water, which, in this Experiment were cooled 78 degrees, actually gave off as much Heat as would have been ſufficient to have melted $\frac{73\frac{1}{4}\times 78}{140}=40\frac{8}{10}$ ounces of ice.

But the quantity of ice actually melted was only about five ounces; and hence it appears that *leſs than one-eighth part of the Heat loſt by the water was communicated to the ice;* the reſt being carried off by the air.

As the ſame quantity of hot water was uſed in this Experiment, and in that, No. 15, which immediately preceded it, and as this water was contained by the ſame veſſel,—(the glaſs jar above deſcribed,)—it appears that ice melts more than *eighty times ſlower* at the bottom of a maſs of boiling-hot water, than when it is ſuffered to ſwim on its ſurface*. For, as in the Experiment, No. 15, $10\frac{1}{8}$ oz. of ice were melted in 2 minutes and 58 ſeconds, 5 ounces at leaſt muſt have been melted in 1 minute and 29 ſeconds; but in the Experiment No. 16, 2 hours or 120 minutes were employed in melting 5 ounces.

The ice however *was melted,* though very ſlowly, at the bottom of the hot water; and that circumſtance alone would have been ſufficient to have overturned my hypotheſis reſpecting the manner in which Heat is propagated in liquids, had I not found means to account in a ſatisfactory manner for that fact, without being obliged to abandon my former opinions.

In about half an hour after the hot water had been poured into the jar, in the laſt Experiment, examining the ſurface of the ice I diſcovered an appearance which fixed my attention and excited all my curioſity; I perceived that the ice had been melted and diminiſhed at its ſurface, *excepting only where it had been covered, or as it were* SHADOWED,

by

by the flat ſlips of deal by which the cake of ice was faſtened down in its place.

Had the ice been protected and prevented from being melted by that piece of the wood *only*, which, being undermoſt of the two, repoſed immediately on the ſurface of the ice, I ſhould not perhaps have been much ſurpriſed; but that part of the ſurface of the ice being likewiſe protected which was ſituated immediately under the other piece of wood,—a piece which, lying acroſs the under piece, and reſting on it, *did not touch the ice any where except juſt at its edge*,—that circumſtance attracted my attention; and I could at firſt ſee no way of accounting for theſe appearances but by ſuppoſing that the ice had been melted by the *calorific rays* which had been emitted by the hot water; and that thoſe parts of the ice which had been *ſhadowed* by the pieces of deal, receiving none of thoſe rays, had of courſe not been melted.

I was ſo much ſtruck with theſe appearances, that I immediately made the following Experiments, with a view merely to their further elucidation of this matter.

Experiment, No. 17.

Into a cylindrical glaſs jar, 6½ inches in diameter, and 8 inches high, I put a circular cake of ice, as large as could be made to enter the jar, and about 3½ inches thick; and on the flat and even ſurface of the ice I placed a circular plate of the thinneſt tin I could procure, near 6½ inches in diameter, or ſufficiently large juſt to cover the ice. This plate of tin (which, to preſerve its form, or keep it quite flat, was ſtrengthened by a ſtrong wire, which went round it at its circumference) had a circular hole in its centre, juſt two inches in diameter, and it was firmly fixed down on the upper ſurface of the cake of ice, by means of ſeveral thin wooden wedges which paſſed between its circumference and the ſides of the jar.

A ſecond circular plate of tin, with a circular hole in its centre two inches in diameter, and in all other reſpects exactly like that already deſcribed, was now placed over the firſt, and parallel to it, at the diſtance of juſt one inch, and like the firſt was firmly fixed in its place by wooden wedges.

Theſe perforated circular plates being fixed in their places, the jar was placed in a room where Fahrenheit's thermometer ſtood at 34°; and ice-cold water was poured into it till the water juſt covered the upper plate; and then the jar was filled to within half an inch of its brim with boiling water: and being covered over with a board, was ſuffered to remain quiet two hours.

At the end of this time, the water, which was ſtill warm, was poured off, and the circular plate being removed, the ice was examined.

A circular excavation, juſt as large as the hole in the tin-plate which covered the ice, (namely two inches in diameter,) and correſponding with it,—perfectly well defined,—and about $\frac{2}{10}$ of an inch deep in the centre, had been made in the ice.

This was what I expected to find; but there was ſomething more, which I did not expect, and which, for ſome time, I was quite at a loſs to account for. Every part of the ſurface of the ice which had been covered by the tin plate, appeared to be perfect, level, and ſmooth, and ſhowed no ſigns of its having been melted or diminiſhed, excepting only in one place, where a channel, about an inch wide, and a little more than $\frac{2}{10}$ of an inch deep, which ſhowed evident marks of having been formed by a ſtream of warm water, led from the excavation juſt mentioned, in the centre of the upper part of the cake of ice, to its circumference.

As the edge, or vertical ſide, of the cake of ice was evidently worn away where this ſtream paſſed, there could be no doubt with reſpect to its direction. It certainly ran *out of* the circular excavation in the middle of the ice; and though it might at firſt appear difficult to explain the fact, and to ſhow how this hot water could arrive at that place, yet it was quite evident that the immediate cauſe of the motion of this ſtream of water could be no other than its ſpecific gravity being greater than that of the reſt of the water at the ſame depth: and that this greater ſpecific gravity was at the ſame time accompanied by a higher degree of Heat, was evident from the deep channel which this ſtream had melted in the ice, while other parts of the ſurface of the ice, at the ſame level, were not melted by the water which reſted on it. To elucidate this matter ſtill farther, I made the following Experiment:

Experiment, No. 18.

Thinking it probable, that if the circular excavation in the ice, which anſwered to the circular hole in the middle of the tin-plate which covered the ice, and alſo to that in the ſecond plate which was placed an inch higher, had been melted by *radiant Heat*, (as it has improperly been called,) or by the calorific rays from the hot water; then, in that caſe, as ſome of theſe rays muſt probably have been reflected downwards at the ſurface of the water, in attempting to paſs upwards into the air, I thought that by preventing this part of them from reaching the ice, which I endeavoured to do by cauſing them to be abſorbed by a light black body, (a circular piece of deal board, covered over with black ſilk,) which I cauſed to ſwim on the ſurface of the water, their effects in melting the ice might perhaps be ſenſibly diminiſhed. Had this really been the caſe, it would certainly have afforded ſtrong grounds to ſuſpect that theſe rays were in fact the cauſe of the appearances in queſtion; but on making the Experiment with the greateſt care, I could not perceive that the covering of the ſurface of the hot water with a black body produced any difference whatever in the reſult of the Experiment as it was firſt made, (Experiment No. 17,) or when this black covering was not uſed.

After ſome meditation on the ſubject, it occurred to me that this melting of the ice at its upper ſurface could be accounted for, in a manner which appeared to me to be perfectly ſatisfactory; without ſuppoſing either that water is a conductor of Heat, or that the effect in queſtion was produced by calorific rays.

Though it is one of the moſt general laws of nature with which we are acquainted, that all bodies, ſolids as well as fluids, are condenſed by cold: yet, in regard to *water*, there appears to be a very remarkable exception to this law. Water, like all other known bodies, is indeed condenſed by cold at every degree of temperature which is conſiderably higher than that of freezing, but its condenſation, on parting with Heat, does not go on till it is changed to ice; but when in cooling its temperature has reached to about the 40th degree of Fahrenheit's ſcale, or about eight degrees above freezing, it ceaſes to be farther condenſed; and on being cooled ſtill farther, *it actually expands*, and continues to expand, as it goes on to looſe more of its Heat, till at laſt it freezes; and at the moment when it becomes ſolid, and even after it has become ſolid, it expands ſtill more, on growing colder.

This fact, which is noticed by M. DE LUÇ, in his excellent treatiſe on the modifications of the atmoſphere, has ſince been farther inveſtigated and put beyond all doubt, by SIR CHARLES BLAGDEN. See Philoſophical Tranſactions, vol. lxxviii.

Now, as water in contact with melting ice is always at the temperature of 32°, it is evident that water *at that temperature* muſt be ſpecifically lighter than water which is eight degrees warmer, or at the temperature of 40°; conſequently, if two parcels of water at theſe two temperatures be contained in the ſame veſſel, that which is the *coldeſt* and *lighteſt* muſt neceſſarily give place to that which is warmer and heavier, and currents of the warmer water will *deſcend* in that which is colder.

In the two laſt Experiments, as the circular tin-plate which covered the ſurface of the ice ſerved to confine the thin ſheet of water which was between the plate and the ice, as this water could not riſe upwards, being hindered by the plate, and as it had no tendency to deſcend, it is probable that it remained in its place; and as it was *ice-cold*, it was not capable of melting the ice on which it repoſed.

But as the tin-plate had a circular hole in its centre, the ſurface of the ice *in that part* was of courſe naked, and the ice-cold water in contact with it being diſplaced by the *warmer* and *heavier* water from above, an excavation, in the form of a ſhallow baſin, was formed in the ice by this *deſcending warm current.*

The warm water contained in this baſin overflowed its banks as ſoon as the baſin began to be formed; and iſſuing out on that ſide which happened to be the loweſt, opened itſelf a paſſage under the tin-plate to the edge of the ice, over which it was precipitated, and fell down to the bottom of the jar. The water of this rivulet being warm, it ſoon formed for itſelf a deep channel in the ice; and at the end of the Experiment it was found to be everywhere *deeper* than the bottom of the baſin where it took its riſe.

This manner of accounting for the appearances in queſtion ſeemed to me to be quite ſatisfactory; and the more I meditated on the ſubject, the more I was confirmed in my ſuſpicions that *all liquids* muſt neceſſarily be perfect *non-conductors of Heat.*

On theſe principles I was now enabled to account for the melting of the ice at the bottom of the hot water in the Experiment No. 16; as alſo for the ſlowneſs with which that proceſs went on;—and encouraged by this ſucceſs, I now proceeded with confidenee to plan and to execute ſtill more deciſive Experiments; from the reſults of which, I may venture to ſay it,—the important facts in queſtion have been put beyond all poſſibility of doubt.

If water be in fact a perfect *non-conductor* of Heat,—that is to ſay, if there be *no communication whatever of Heat* between neighbouring particles or *molécules* of that fluid, (which is what I ſuppoſe,) then,—as Heat cannot be propagated in it but *only* in conſequence of the motions occaſioned in the fluid by the changes in the ſpecific gravity of thoſe particles which are occaſioned by the changes of their temperature, it follows that Heat cannot be propagated *downwards* in water, as long as that fluid continues to be condenſed with cold; and that it is *only in that direction,* (downwards,) that it *can be propagated* after the water has arrived at that temperature, where it begins to be expanded by cold;—which has been found to be at about the 40th degree of Fahrenheit's ſcale.

Reaſoning on theſe principles, we are led to this remarkable concluſion; namely, that *water which is only eight degrees above the freezing point, or at the temperature of* 40°,—*muſt be able to melt as much ice in any given time*, WHEN STANDING ON ITS SURFACE, *as an equal volume of water at any higher temperature*, EVEN THOUGH IT WERE BOILING HOT.

My philoſophical reader will doubtleſs think that I muſt have had no ſmall degree of confidence in the opinion I had formed on this intereſting ſubject, to have had the courage to make, *even in private*, the Experiments that were neceſſary to aſcertain that fact.

Experiment, No. 19.

Into a cylindrical glaſs jar, 4.7 inches in diameter, and 13.8 inches high, I put 43.87 cubic inches, or 1 lb. 11$\frac{1}{7}$ oz. Troy, in weight, of water, and placing the jar in a freezing mixture, compoſed of pounded ice and common ſea-ſalt, I cauſed the water to freeze into one compact maſs; which adhered firmly to the bottom and ſides of the jar, and which formed a cylinder of ice juſt three inches high.

Had the bottom of the jar been quite flat, inſtead of being raiſed, or vaulted, the cylinder of ice would have been no more than 2.67 inches high.

As ſoon as the water in the jar was completely frozen, the jar was removed from the freezing mixture, and placed in a mixture of pounded ice and pure water, where it was ſuffered to remain four hours, in order that the cake of ice in the jar might be brought to the preciſe temperature of 32°.

The jar ſtill ſtanding in a ſhallow diſh in the pounded ice and water, the ſurface of which cold mixture was juſt on a level with the ſurface of the ice in the jar, I covered the top of the cake of ice with a circular piece of ſtrong paper, and poured gently into the jar 73¼ oz. Troy of boiling-hot water, which filled it to the height of eight inches above the ſurface of the ice. (See Plate II.)

I then removed very gently the circular piece of paper which covered the ſurface of the ice, and after leaving the hot water in contact with the ice a certain number of minutes, I poured it off, and —weighing immediately the jar, and the unmelted ice which remained in it,—I aſcertained the *quantity of ice which had been melted* by the hot water during the time it had been ſuffered to remain in the jar.

This Experiment I repeated four times the ſame day, (16th March 1797,) varying at each repetition of it the time the water was permitted to remain on the ice. The reſults of theſe Experiments were as follows:

Number of the Experiment.	Time the hot water remained on the ice.	Temperature of the hot water when it was poured on the ice.	Temperature of the water 1 inch below its ſurface at the end of the Experiment.	Quantity of ice melted.
	Minutes.			Grains.
No. 19	1	186°	Not obſerved	1632
No. 20	3¾	185°	Not obſerved	1824
No. 21	15	184°	170°	1757
No. 22	60	186°	140°	2573

From the reſults of theſe Experiments, it was plain that a very conſiderable portion of the ice that was melted, was melted in the very beginning of the Experiments, or while the hot water was actually *pouring* into the jar; which operation commonly laſted about one minute: and the irregularities in the reſults of the Experiments, and particularly of the three firſt, ſhowed evidently that the quantity of ice melted in that operation was different in different Experiments. I had indeed foreſeen that this would be the caſe; and on that account it was that I covered the ſurface of the ice with a circular piece of ſtrong paper, and always took care to pour the water very gently into the jar: but I found that all theſe precautions were not ſufficient to prevent very conſiderable anomalies in the reſults of the Experiments; and as I found reaſon to ſuſpect that the motion in the maſs of the hot water, that was unavoidably occaſioned by removing the circular piece of paper which covered the ice, was the principal cauſe of theſe inaccuracies, I had recourſe to another, and a better contrivance.

Having procured a flat, ſhallow diſh, of light wood, half an inch deep, 4½ inches in diameter, (or ſomething leſs than the internal diameter of the jar,)—and about ¼ of an inch thick at its bottom, I bored a great number of very ſmall holes through its bottom, which gave it the appearance of a ſieve. This perforated wooden diſh having been previouſly made *ice-cold*, was placed on the ſurface of the ice in the jar; and the hot water being gently poured into the diſh through a long wooden tube; as this perforated diſh floated and remained conſtantly at the ſurface of the water, and as the water paſſing through ſuch a great number—(many hundreds)—of ſmall holes, was not projected downwards with force, it is evident that by this ſimple contrivance thoſe violent motions in the maſs of water in the jar, that before took place when the hot water was poured into the ice, and when the paper which covered the ice was removed, were, in a great meaſure, prevented.

In order that the water that was poured through the wooden tube (the bore of which was about half an inch in diameter) might not impinge perpendicularly and with force againſt the bottom of the diſh, the lower end of the tube was cloſed by a fit cork-ſtopple, and the water was made to iſſue horizontally through a number of ſmall holes in the ſides of this tube, at its lower end.

As ſoon as the operation of pouring the hot water into the jar was finiſhed, the perforated diſh was carefully removed, and the jar was covered with a circular wooden cover, from the centre of which a ſmall mercurial thermometer was ſuſpended.

The effects produced by this new arrangement of the machinery will appear by comparing the reſults of the two following Experiments with thoſe juſt mentioned.

Number of the Experiment.	Time the hot water remained on the ice.	Temperature of the hot water,		Quantity of ice melted.
		At the beginning.	At the end.	
	Minutes.			Grains.
No. 23	1	196°	196°	423
No. 24	3	190°	188°	703

In order ſtill more effectually to prevent the inaccuracies ariſing from the internal motions in the maſs of hot water that were occaſioned in pouring the water into the jar, (and which could not fail to affect, more or leſs, the reſults of the Experiment,) I had recourſe to the following contrivance.

I filled a ſmall phial containing 8¼ cubic inches with ice-cold water, and then emptying the phial in the jar, I covered the ſurface of the ice with this ice-cold water to the height of 0.478 of an inch.

On the ſurface of this ice-cold water, inſtead of that of the ice, I now placed the perforated wooden diſh, previouſly made ice-cold, and poured the hot water upon it.

The reſults of the following Experiments ſhow that this contrivance tended much to diminiſh the apparent irregularities of the Experiments.

The air of the room in which theſe Experiments were made was at the temperature of 41°.

No. of the Experiment.	Time the hot water was on the ice.	Temperature of the hot water one inch below its ſurface,		Quantity of ice melted.
		At the beginning	At the end.	
	Minutes.			Grains.
No. 25	10	192°	182°	580
No. 26	30	190°	165°	914
No. 27	180	190°	95°	3200

From the reſults of theſe laſt three Experiments we can now determine with a very conſiderable degree of certainty how much ice was melted *in the act of pouring the water into the jar*, and conſequently the rate at which it was melted in the ordinary courſe of the Experiment;—ſuppoſing equal quantities to be melted in equal times.

As in the 27th Experiment 3200 grains were melted in 180 minutes, and in the 25th Experiment 580 grains were melted in 10 minutes, we may ſafely conclude that the ſame quantity muſt have been melted in the ſame time (10 minutes) in the 27th Experiment; if, therefore, from 3200 grains,—the quantity melted in 180 minutes in this laſt Experiment,—we deduct 580 grains for the quantity melted during the firſt 10 minutes,—there will remain 2620 grains for the quantity melted in the ſucceeding 170 minutes, when, the motions occaſioned in the water on its being poured into the jar having ſubſided, we may ſuppoſe the proceſs of melting the ice to have gone on regularly.

But if in the regular courſe of the Experiment, no more than 2620 grains were melted in 170 minutes, it is evident that not more than 154 grains could have been melted in the ordinary courſe of the proceſs in 10 minutes; for 170 minutes: 2620 grains : : 10 minutes: 154 grains.—If, therefore, from 580 grains, the quantity of ice actually melted in 10 minutes in the 25th Experiment, we deduct 154 grains, there remains 426 for the quantity melted in pouring the water into the jar.

Let us ſee now how far this agrees with the reſult of the 26th Experiment. In this Experiment 914 grains of ice were melted in 30 minutes. If from this quantity we deduct 426 grains, the quantity, which, according to the foregoing computation, muſt have been melted *in pouring the hot water into the jar*, there will remain 478 grains for the quantity melted in the ordinary courſe of the proceſs in 30 minutes; which gives 159 grains for the quantity melted in 10 minutes; which differs very little from the reſult of the foregoing computation, by which it appeared to be = 154 grains. This difference however, ſmall as it is, is ſufficient to prove an important fact, namely, that the effects produced by the motion into which the hot water had been thrown in being poured into the jar had not ceaſed entirely in 10 minutes, or when an end was put to the 5th Experiment. We ſhall therefore come nearer the truth, if, in our endeavours to diſcover the quantity of ice melted in any given time, *in the ordinary courſe of the Experiments*, we found our computation on the reſults of the two Experiments No. 26 and No. 27.

In the latter of theſe Experiments 3200 grains of ice were melted in 180 minutes, and in the former 914 grains were melted in 30 minutes. If, therefore, from 3200 grains, the quantity melted in 180 minutes, we take the quantity melted in the firſt 30 minutes, =914 grains, there will remain 2286 grains for the quantity melted in the ſucceeding 150 minutes, and this gives 150 grains for the quantity melted in 10 minutes. By the former computation it turned out to be 154 grains.

But if 152 grains of ice is the quantity melted in 10 minutes, in the ordinary courſe of the proceſs, three times that quantity, or 456 grains only, could have been melted *in this manner* in the 30 minutes during which the 26th Experiment laſted; and deducting this quantity from 914 grains, the quantity actually melted in that Experiment, the remainder, 458 grains, ſhows how much muſt have been melted in the pouring the hot water on the ice,—or in conſequence of the motions into which the water was thrown in the performance of that operation. By the preceding computation this quantity turned out to be 426 grains.

From the reſult of all theſe computations I think we may ſafely conclude, that in the ordinary courſe of the Experiments not more than 152 grains of ice were melted by the hot water in 10 minutes.

I ſhall now proceed to give an account of ſeveral Experiments in which the water employed to melt the ice was at a *much lower temperature.*

Having removed a ſmall quantity of ice which remained unmelted in the bottom of the jar, I put a freſh quantity of water into it, and placing the jar in a freezing mixture, cauſed this water, which filled the jar to the height of four inches, to freeze into one ſolid maſs of ice. I then placed the jar in a ſhallow earthen diſh, and ſurrounded it to the height of the level of the top of the ice with a mixture of ſnow and water (ſee Plate II.); and placing it in a room in which there had been no fire made for many months, and in which the temperature of the air was at 41°, I let it remain quiet two hours, in order that the ice might acquire the temperature of 32°.

This being done, I took the jar out of the earthen diſh, and wiping the outſide of it dry with a cold napkin, I weighed the jar with the ice in it very exactly, and then replaced it in the earthen diſh, and ſurrounded it as before with ſnow and water, to the height of the level of the ſurface of the ice.

I then poured 73¼ ounces Troy (=15,160 grains) of water, at the temperature of 41°, into the jar, which covered the ice to the ſame height to which it had been covered in the former Experiment,—namely, to about 8 inches; and ſuffering it to ſtand on the ice a certain number of minutes, I then poured it off, and wiping the outſide of the jar, weighed it a ſecond time, in order to aſcertain how much ice had been melted.

In putting this cold water into the jar, the ſame precautions were uſed—(by pouring it through the wooden tube into the perforated wooden diſh, &c.)—as were uſed when the Experiment was made with boiling the water.

The following Table ſhows the reſults of ſix Experiments made the ſame day, (the 19th March 1797,) in the order in which they are numbered, and which were all made with the utmoſt care:

Number of the Experiment.	Temperature of the water in the jar 1 inch below its ſurface, At the beginning of the Exp.	Temperature of the water in the jar 1 inch below its ſurface, At the end of the Exp.	Temperature of the air.	Time the water remained on the ice.	Quantity of ice melted.
				Minutes.	Grains.
No. 28	41°	40°	41°	10	203
No. 29	41°	40°	41°	10	210
No. 30	41°	40°	41°	10	237
No. 31	41°	40°	41°	10	228
No. 32	41°	38°	41°	30	617
No. 33	41°	38°	41°	30	585

The agreement in the reſults of theſe Experiments is not much leſs extraordinary than the ſurpriſing fact which is proved by them,—namely, that boiling-hot water does not thaw more ice in any given time *when ſtanding quietly on its ſurface*, than water at the temperature of 41°—or nine degrees only above the point of freezing!

There is reaſon to conclude that it does not even thaw ſo much;—and this ſtill more remarkable circumſtance may, I think, be accounted for in a ſatisfactory manner, on the ſuppoſition (which, however, I imagine will no longer be conſidered as a bare a bare ſuppoſition) that water is a non-conductor of Heat.

It appeared from the reſults of the Experiments made with hot water, that the quantity of ice melted in 10 minutes in the ordinary courſe of that proceſs amounted to no more than 152 grains;—but in theſe Experiments with cold water, the quantity melted in that time was never leſs than 203 grains, and taking the mean of four Experiments, it amounted to 222 grains.

There is one circumſtance, however, reſpecting theſe Experiments with cold water, which it is neceſſary to inveſtigate before their reſults can be admitted as complete proof in the important caſe in queſtion.

In the Experiments which were made with hot water, it was found that a conſiderable part of the ice which was melted, was melted in conſequence of the motions into which the water was thrown upon being poured into the jar, and that the effect of theſe motions continued to be ſenſible for a longer time than moſt of theſe Experiments with cold water laſted. Is it not poſſible that the reſults of theſe Experiments with cold water may alſo have been affected by the ſame cauſe? This is what I ſhall endeavour to find out.

In the 32d Experiment 617 grains of ice were melted in 30 minutes, and in the 33d Experiment 585 grains were melted in the ſame time; and taking the mean of theſe two Experiments it appears that 601 grains were melted in 30 minutes.

If now from this quantity we deduct that which according to the mean reſult of the four preceding Experiments, muſt have been melted in 10 minutes, namely, 222 grains, there will remain 379 grains for the quantity melted in the laſt 20 minutes in theſe two Experiments; conſequently, half this quantity, or 189½ grains, is what muſt have been melted in 10 minutes *in the ordinary courſe of the proceſs*.

But this quantity, (189½ grains,) though leſs than what was actually melted in the Experiments which laſted only 10 minutes, is ſtill conſiderably greater than 152 grains, the quantity which was found to have been melted in the ſame time *in the ordinary courſe of the proceſs* in thoſe Experiments in which *hot water* was uſed;—conſequently the great queſtion, for the deciſion of which theſe Experiments were contrived, is,—I believe I may venture to ſay,—*decided**.

But, however concluſive the reſult of theſe Experiments appeared to me to be, I felt myſelf too much intereſted in the ſubject to reſt my inquiries here.

Having found, as well from the reſults of the Experiments made with cold water, as from thoſe made with hot water, that a conſiderable quantity of ice was melted in the act of pouring the water into the jar, and in conſequence of thoſe undulatory motions into which the water was thrown in that operation, notwithſtanding all the pains I had taken to diminiſh thoſe motions, and prevent their effects, I now doubled my precautions in guarding againſt thoſe ſources of error and uncertainty.

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* Thoſe who refuſe their aſſent to the concluſion here drawn,—namely, that there is not any communication of Heat whatever between neighbouring particles of water, are deſired to explain the Phenomena on their own principles, and ſhew how it happens that BOILING-HOT water thaws LESS ice in any given time, when ſtanding on its ſurface, than COLD-WATER.

Before I poured the water into the jar I covered the ſurface of the ice to the height 0.956 of an inch with ice-cold water, and this I did when water at the temperature of 41° was uſed, as well as in thoſe Experiments in which boiling-hot water was employed. In the former Experiments I had covered the ſurface of the ice with ice-cold water only in thoſe Experiments in which hot water was uſed, and even in thoſe I uſed only half as much ice-cold water as I now employed for that purpoſe.

I alſo now poured the water into the jar in a ſmaller ſtream, employing no leſs than three minutes in filling it up to the height of eight inches above the ſurface of the ice; and I endeavoured to aſcertain how far the reſults of the Experiments were influenced by the temperature of the air, and alſo by wrapping up the jar in warm covering.

The ſame jar was uſed in all the Experiments, and it was always placed in the ſame earthen diſh, and ſurrounded, to the level of the top of the ice, with melting ſnow. This jar is very regular in its form, being very nearly a perfect cylinder, and is on that account peculiarly well calculated for the uſe for which I ſelected it.

In each of the three firſt Experiments, which are entered in the following Table, the jar was well covered up with a very warm covering of cotton wool. This covering (which was above an inch thick) reached from the ſurface of the melting ſnow in which the jar ſtood, quite to the top of the jar. The mouth of the jar was firſt covered with a round wooden cover, (from the centre of which a thermometer, the bulb of which reached one inch below the ſurface of the water, was ſuſpended) and on the top of this wooden cover there was put a thick covering of cotton.

In all the Experiments in the following Table, except the three firſt, the jar was expoſed naked to the air, except the lower part of it, which, as I have already more than once obſerved, was always covered, as high as the ice in the jar reached, with melting ſnow, or with pounded ice and water.

In the two Experiments No. 37 and No. 38, which are marked with aſteriſks, the ſurface of the ice was covered with ice-cold water to the depth of 0.478 of an inch only;—in all the other Experiments it was covered to the depth of 0.956 of an inch.

Number of the Experiment.	Temperature of the water in the jar, 1 inch below its ſurface,		Temperature of the air.	Time the water remained on the ice.	Quantity of ice melted.
	In the beginning of the Exp.	At the end of the Exp.			
				Minutes.	Grains.
No. 34	188°	179°	41°	30	634
No. 35	189°	180°	41°	30	747
No. 36	190°	147°	41°	180	3963
No. 37	41°	38°	41°	30	592*
No. 38	41°	43°	61°	30	676*
No. 39	186°	157°	61°	30	559
No. 40	188°	156°	61°	30	575
No. 41	190°	156°	61°	30	542
No. 42	41°	43°	61°	30	573
No. 43	42°	44°	61°	30	575
No. 44	42°	35°	61°	120	2151

The reſults of theſe Experiments afford matter for much curious ſpeculation, but I ſhall content myſelf for the preſent with making only two or three obſervations reſpecting them. And in the firſt place, it is remarkable, that although in the Experiments No. 34 and No. 35, of 30 minutes each, conſiderably leſs ice was melted than in that No. 26, which laſted the ſame time; yet, in that No. 36, of 180 minutes, more was melted than in that No. 27, of the ſame duration. This difference in the two laſt-mentioned Experiments will be accounted for hereafter.

With regard to the difference in the reſults of the Experiments of 30 minutes, there is no doubt but that it aroſe from the precautions which had been taken in this laſt ſet of Experiments to prevent the effect of the violent motions into which the hot water was thrown in being poured into the jar, that leſs ice was melted in the Experiments No. 34 and No. 35, than in that No. 26.

It appears that more ice was melted in the ſame time, in the Experiments in which the jar was covered up with warm covering, than in thoſe in which it was left naked and expoſed to the air of the room. The difference is even conſiderable. The quantity melted in 30 minutes when the jar was covered, at a mean of two Experiments, (No. 34 and No. 35,) was 690½ grains;—but when the jar was naked, the quantity at a mean of three Experiments (No. 39, No. 40, and No. 41) was only 558⅓ grains.

The quantity of ice melted under ſimilar circumſtances,—that is to ſay, when the jar was naked,—*was ſenſibly greater when the water was at the temperature of about* 41°, *than when it was nearly boiling hot.* In the Experiment No. 41, when the water which was poured on the ice was at the temperature of 190 deg. 542 grains only of ice were melted in 30 minutes; whereas, in the next Experiment, (No. 42,) when the water was at 41°, or 149 degrees colder, 573 grains were melted in the ſame time.

Finding that covering up the jar with a thick and warm covering of cotton cauſed more ice to be melted by the hot water, I was curious to ſee what effects would be produced by keeping the jar plunged *quite up to its brim* in a mixture of ſnow and water, inſtead of merely ſurrounding that part of it which was occupied by the cake of ice by this cold mixture.

I was likewiſe deſirous of finding out,—and if poſſible at the ſame time,—whether water at a temperature ſomething above that at which that Fluid ceaſes to be condenſed with cold, would not melt more ice in any given time than an equal quantity of that Fluid, either colder or much hotter. The reſult of the 43d Experiment had ſhewn me, —what indeed a very ſimple computation would have pointed out,—namely, that when the temperature of the water is but a few degrees above the point of freezing, if its quantity or depth is not very conſiderable, it will ſoon be ſo much cooled as very ſenſibly to retard the proceſs of melting the ice: and with reſpect to hot water, the increaſed quantity of ice which was melted by it when the jar was covered up with a warm covering, convinced me that the real cauſe which prevented the hot water from melting as much ice as the cold water, in my Experiments, was, the embarraſſments in the proceſs of melting the ice, which were occaſioned *by the deſcending currents formed in the hot water on its being cooled by the air at its ſurface, and by a contact with the ſides of the jar.*

Theſe deſcending currents meeting in the region of the conſtant temperature of 40° with thoſe cold currents which aſcended from the ſurface of the ice, it ſeems very probable that the aſcending currents,—on the motion of which the melting of ice depends,—were checked by this colliſion.

By retarding the cooling of the hot water above, by wrapping up the jar in a warm covering, the velocity of the deſcending currents was of courſe diminiſhed;—but when this was done, the reſults of the Experiment ſhowed that the melting of the ice was accelerated.

When, the jar being naked, the cooling of the hot water, and conſequently the motions of the deſcending currents, were rapid, no more than about 542 grains, or at moſt 575 grains, were melted in 30 minutes; but when the jar was covered with a warm covering, 634 grains, and in one Experiment (that No. 35) 747 grains, were melted in the ſame time.

As plunging the jar into a cold mixture of ſnow and water could not fail to accelerate the cooling of the hot water in the jar, and conſequently to increaſe the rapidity of the deſcending currents in it, ought not this to embarraſs, in an extraordinary degree, the aſcending currents of ice-cold water from the ſurface of the ice, and to diminiſh the quantity of ice melted?—This is what the following Experiments, compared with the reſults of thoſe No. 39, No. 40, and No. 41, will ſhow.

Number of the Experiment.	Temperature of the water in the jar 1 inch below its ſurface,		Temperature of the cold mixture in which the jar was kept plunged to its brim.	Time the water remained on the ice.	Quantity of ice melted.
	In the beginning of the Exp.	At the end of the Exp.			
				Minutes.	Grains.
No. 45	188°	68°	32°	30	406
No. 46	186°	67°	32°	30	440
No. 47	189°	68°	32°	30	432
No. 48	187°	67°	32°	30	355
No. 49	188°	68°	32°	30	364
Quantity of ice melted in theſe 5 Experiments,					1997
					Grains.
Mean quantity melted by *hot water* when the jar was kept plunged to its brim in melting ice and water,					$399\frac{2}{5}$
Mean quantity melted by *hot water* in 30 minutes, in the two Experiments, No. 26 and No. 27, when the part of the jar occupied by the water was ſurrounded by air, at the temperature of 41°, - -					456
Mean quantity melted by *hot water* in 30 minutes, in the three Experiments, No. 39, No. 40, and No. 41, when the part of the jar occupied by the water was ſurrounded by air, at the temperature of 61°, -					$558\frac{2}{3}$
Mean quantity melted by *hot water* in 30 minutes, in the two Experiments, No. 34 and No. 35, when the part of the jar occupied by the water was kept covered up by a thick and warm covering of cotton, -					$690\frac{1}{2}$

As all the Experiments were made in the ſame manner, and with equal care, and differed only in reſpect to the manner in which the outſide of the jar, above the ſurface of the ice in it, was covered, their reſults ſhow the effects produced by thoſe differences.

I ſhould perhaps have ſuſpected that the greater quantity of ice which was melted when the heat of the water in the jar was confined for the longeſt time had been occaſioned, at leaſt in part, by the Heat communicated downwards by the medium of the glaſs; but that this could not have been the caſe was evident, not only from the manner in which the ice was always found to have been melted, but alſo from the reſults of ſimilar Experiments made with much colder water.

Had it been melted by Heat communicated by the glaſs, it would undoubtedly have been moſt melted in thoſe parts of its ſurface where it was in contact with the glaſs, but this I never once found to be the caſe.

The reſults of the following Experiments will ſhow,—what indeed might eaſily have been foreſeen,—that the temperature of the medium by which the upper part of the jar was ſurrounded did not always affect the reſult of the Experiment in the ſame degree,—nor even always in *the ſame manner*,—in different Experiments in which the temperature of the water in the jar was very different.

To facilitate the compariſon of theſe Experiments, and that of the foregoing, which are ſimilar to them, I ſhall here place them together.

Number of the Experiment.	Temperature of the water in the jar, 1 inch below its ſurface,		Temperature of the medium by which the upper part of the jar was ſurrounded.	Time the water remained on the ice.	Quantity of ice melted.
	In the beginning of the Exp.	At the end of the Exp.			
				Grains.	Minutes.
No. 50	41°	36°	32°	30	542
No. 37	41°	38°	41°	30	592
No. 42,	41°	43°	61°	30	576

It is certainly very remarkable indeed that ſo much more ice ſhould be melted by water at the temperature of 41°, when the jar which contained it was ſurrounded by a cold mixture of pounded ice and water, than by an equal quantity of boiling-hot water in the ſame circumſtances. In the Experiment No. 50, the quantity melted by the cold water was 542 grains, while that melted by the boiling-hot water, taking the mean of five Experiments, (thoſe No. 45, 46, 47, 48, and 49,) was no more than $399\frac{1}{5}$ grains. But the reſults of the four following Experiments are, if poſſible, ſtill more ſurpriſing.

Theſe Experiments were made with water at the temperature of 61°, the temperature of the air of the room being at the ſame time 61°; in the two firſt of theſe Experiments the jar was kept plunged to its brim in a mixture of ſnow and water,—in the two laſt its lower part only,—namely, as high as the level of the ſurface of the ice,—was ſurrounded by this cold mixture; its upper part being naked, and ſurrounded by the air of the room.

In each of the Experiments, (as in thoſe which preceded them,) before the water was poured into the jar, the ſurface of the ice was covered to the height of 0.956 of an inch with ice-cold water, in order more effectually to defend it againſt the effects of the temporary motions into which the water employed to melt the ice was unavoidably thrown in the performance of this operation; and the ſame quantity of water was always uſed in the different Experiments, namely, 73¼ ounces Troy; or as much as was ſufficient to fill the jar to the height of 8 inches.

Number of the Experiment.	Temperature of the water in the jar 1 inch below its ſurface,		Temperature of the medium by which the upper part of the jar was ſurrounded.	Time the water remained on the ice.	Quantity of ice melted.
	In the beginning of the Exp.	At the end of the Exp.			
				Minutes.	Grains.
No. 51	61°	49°	32°	30	660
No. 52	61°	50°	32°	30	662
No. 53	61°	60°	61°	30	642
No. 54	61°	60°	61°	30	650

Theſe Experiments are remarkable, not only on account of the very ſmall difference in the quantities of ice melted, which reſulted from the cooling of the ſides of the jar, but alſo, and more eſpecially, as that difference was directly contrary to the effects produced by the ſame means in the Experiments with hot water. More ice was melted when the outſide of the jar was kept ice-cold, than when it was ſurrounded by air at the temperature of 61°.

All theſe appearances might, I think, be accounted for in a ſatisfactory manner, on the principles we have aſſumed reſpecting the manner in which Heat is propagated in liquids; but without engaging ourſelves at preſent too far in theſe abſtruſe ſpeculations, let us take a retroſpective view of all our Experiments, and ſee what general reſults may with certainty be drawn from them.

One of the Experiments in which the greateſt quantity of ice was melted by *hot water* is that No. 36, in which 3963 grains were melted in three hours, or 180 minutes. If now from this quantity we deduct that which, according to the reſults of the two preceding Experiments, muſt have been melted in the firſt 30 minutes, namely, 690½ grains, there will remain 3272½ grains for the quantity melted in the laſt 150 minutes, which gives 654½ grains for the quantity melted in 30 minutes *in the ordinary courſe of the Experiment.*

This quantity, 654½ grains, deducted from that which at a mean of two Experiments (thoſe No. 34 and No. 35) was found to be actually melted in 30 minutes, namely, 690½ grains, leaves 36 grains for the quantity which in theſe two Experiments was melted in conſequence of the temporary motions into which the hot water was thrown in the operation of pouring it into the jar. The difference between theſe two quantities (=36 grains) is very inconſiderable, and ſhows that the means uſed for diminiſhing the effects produced by thoſe motions had been very efficacious.

As the reſults of the three Experiments No. 34, No. 35, and No. 36, were exceedingly regular and ſatisfactory,—as the Heat of the water appears to have been ſo completely confined by the warm covering which ſurrounded the jar, and as the proceſs of melting the ice went on regularly or equally for ſo great a length of time (three hours) in the 36th Experiment, we may venture to conclude that more ice could not poſſibly have been melted by boiling-hot water—*ſtanding on it*—than was melted in theſe Experiments.

This quantity was found to be at the rate of 654⅓ grains in 30 minutes.

But as in theſe three Experiments extraordinary means were uſed, by which an uncommonly large quantity of ice was melted, they cannot be conſidered as ſimilar to thoſe which were made with cold water, and conſequently cannot with propriety be compared with them.

When the Experiments were ſimilar, the mean reſults of thoſe which were made with water at different temperatures were as follows:

		Ice melted in 30 minutes.
		Grains.
In the Experiments in which the part of the jar which was occupied by the water was expoſed uncovered to the air at the temperature of 61°	With boiling-hot water (*Experiments* No. 39, 40, *and* 41)	558⅔
	With water at the temperature of 61° (*Experiments* No. 53 *and* No. 54)	646
	With water at the temperature of 41° (*Experiments* No. 42 *and* No. 43)	574
In the Experiments in which the part of the jar which was occupied by the water was ſurrounded by pounded ice and water, and conſequently was at the temperature of 32°	With boiling-hot water, (*Experiments* No. 45, 46, 47, 48, *and* 49)	399⅖
	With water at the temperature of 61° (*Experiments* No. 51 *and* No. 52)	661
	With water at the temperature of 41° (*Experiment* No. 50)	542

From the reſults of all theſe Experiments we may certainly venture to conclude, that boiling-hot water is not capable of melting more ice *when ſtanding on its ſurface*, than an equal quantity of water at the temperature of 41°, or when it is only *nine degrees* above the temperature of freezing!

This fact will, I flatter myſelf, be conſidered as affording the moſt unqueſtionable proof that could well be imagined, that water is a perfect *non-conductor of Heat*, and that Heat is propagated in it *only* in conſequence of the motions which the

Heat occaſions in the inſulated and ſolitary particles of that fluid*.

The diſcovery of this fact opens to our view one of the moſt intereſting ſcenes in the economy of Nature:—but in order to prepare our minds for the contemplation of it, it will be not amiſs to refreſh our memory by recapitulating what has already been ſaid on the Propagation of Heat in Fluids, and particularly in water; and adding ſuch occaſional obſervations as may tend to elucidate that abſtruſe ſubject.

Thoſe who enter into the ſpirit of theſe inveſtigations will not conſider theſe repetitions and illuſtrations as either ſuperfluous or tireſome.

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[*] The inſight which this diſcovery gives us in regard to the nature of the mechanical proceſs which takes place in chemical ſolutions is too evident to require illuſtration;—and it appears to me that it will enable us to account in a ſatisfactorily manner for all the various phænomena of chemical affinities and vegetation. Perhaps all the motions among inanimate bodies on the ſurface of the globe may be traced to the ſame cauſe,—namely, to the non-conducting power of Fluids with regard to Heat.

CHAP. III.

Recapitulation, and farther Inveſtigation of the Subject.—All Bodies are condenſed by Cold, without Limitation, WATER ONLY EXCEPTED.—*Wonderful Effects produced in the World in conſequence of the particular Law which obtains in the Condenſation of Water.—This Exception to one of the moſt general Laws of Nature is a ſtriking Proof of* CONTRIVANCE *in the Arrangement of the Univerſe; a Proof which comes home to the Feelings of every ingenuous and grateful Mind.—This particular Law does not obtain in the Condenſation of* SALT-WATER.—*Final Cauſe of the Saltneſs of the Sea.—The Ocean probably deſigned by the Creator to ſerve as an Equalizer of Heat—Could not have anſwered that Purpoſe had its Waters been freſh.—Final cauſes of the Freſhneſs of Lakes and inland Seas in high Latitudes.—Uſefulneſs of theſe Speculations.*

As the immediate cauſe of the motions in a liquid, which take place on its undergoing a change of temperature, is evidently the change in the ſpecific gravity of thoſe particles of the liquid which become either hotter or colder than the reſt of the maſs; and as the ſpecific gravities of ſome liquids are much more changed by any given change of temperature than that of others, ought not this circumſtance (independent of the more or leſs perfect fluidity of the liquid) to make a ſenſible difference in the conducting power of liquids?

The more a liquid is expanded by any given change of temperature, the more rapid will be the aſcent of the particles which firſt receive the Heat; and as theſe are immediately replaced by other colder particles, which, in their turns, come to be heated, this muſt of courſe produce a rapid communication of Heat from the hot body to the liquid.

But when, on the other hand, the ſpecific gravity of a liquid is but little changed by any given change of temperature, the motions among the particles of the liquid occaſioned by this change muſt be very ſluggiſh, and the communication of Heat of courſe very ſlow.

Let us ſtop here, for one moment, juſt to aſk ourſelves a very intereſting queſtion. Suppoſe that in the general arrangement of things it had been neceſſary to contrive matters ſo that water ſhould not freeze in winter,—or that it ſhould not freeze *but with the greateſt difficulty;*—very ſlowly; *and in the ſmalleſt quantity poſſible;*—How could this have been moſt readily effected?—

Thoſe who are acquainted with the law of the condenſation of Water on parting with its Heat have already anticipated me in theſe ſpeculations; and it does not appear to me that there is any thing which human ſagacity can fathom, within the wide-extended bounds of the viſible creation, which affords a more ſtriking, or more palpable proof of the wiſdom of the Creator, and of the ſpecial care he has taken in the general arrangement of the univerſe to preſerve animal life, than this wonderful contrivance:—for though the extenſiveneſs and immutability of the general laws of Nature impreſs our minds with awe and reverence for the Creator of the univerſe, yet, *exceptions to thoſe laws*, or particular modifications of them, from which we are able to trace effects evidently *ſalutary*, or advantageous to ourſelves and our fellow-creatures, afford ſtill more ſtriking proofs of *contrivance*, and ought certainly to awaken in us the moſt lively ſentiments of admiration, love, and gratitude.

Though, in temperatures above blood-heat, the expanſion of water with Heat is very conſiderable, yet, in the neighbourhood of the freezing point it is almoſt nothing. And what is ſtill more remarkable, —as it is an exception to one of the moſt general laws of Nature with which we are acquainted,— when, in cooling, it comes within eight or nine degrees of Fahrenheit's ſcale of the freezing point, inſtead of going on to be farther condenſed as it loſes more of its Heat, it *actually expands*, as it grows colder, and continues to expand more and more, as it is more cooled.

If the whole amount of the condenſation of any given quantity of boiling-hot water, on being cooled to the point of freezing, be divided into any given number of equal parts, the condenſations correſponding to equal changes of temperature will be very unequal in different temperatures.

In cooling 22½ degrees of Fahrenheit's ſcale, (or one-eighth part of the interval between the boiling and the freezing points,) the condenſation will be,

				Condenſation.
In cooling 22½° viz. from	212°	to	189½°	- 18 parts
	189½°	—	167°	- 16.2
	167°	—	144½°	- 13.8
	144½°	—	122°	- 11.5
	122°	—	99½°	- 9.3
	99½°	—	77°	- 7.1
	77°	—	54½°	- 3.9
	54½°	—	32°	- 0.2

Hence it appears that the condenſation of water, or increaſe of its ſpecific gravity in being cooled 22½ degrees of Fahrenheit's ſcale, is at leaſt *ninety times greater* when the water is boiling-hot, than when it is at the mean temperature of the atmoſphere in England (54½°), or within 22½ degrees of freezing—(for 18 is to 0.2 as 90 to 1).

All liquids, it is true, in cooling, are more condenſed by any given change of temperature when they are very hot, than when they are colder; but theſe differences are nothing compared to thoſe we obſerve in water.

The ratio of the condenſation in cooling from 212° to 189½° to that in cooling from 54½° to 32° in each of the under-mentioned fluids, has been ſhown, by the Experiments of M. DE LUC, to be as follows:

Olive oil - -	as $1\frac{14}{100}$ to 1
Strong ſpirits of wine -	as $1\frac{29}{100}$ to 1
A ſaturated ſolution of ſea-ſalt in water -	as $1\frac{38}{100}$ to 1
Pure water - -	as 90 to 1

The difference between the laws of the condenſation of *pure water*, and of the ſame fluid when it holds in ſolution a portion of ſalt, is ſtriking; but when we trace *the effects* which are produced in the world by that arrangement, we ſhall be loſt in wonder and admiration.

Let me beg the attention of my reader while I endeavour to inveſtigate this moſt intereſting ſubject, and let me at the ſame time beſpeak his candour and indulgence. I feel the danger to which a mortal expoſes himſelf who has the temerity to undertake to explain the deſigns of Infinite Wiſdom.—The enterpriſe is adventurous, but it cannot ſurely be improper.

The wonderful ſimplicity of the means employed by the Creator of the world to produce the changes of the ſeaſons, with all the innumerable advantages to the inhabitants of the earth, which flow from them, cannot fail to make a very deep and a laſting impreſſion on every human being whoſe mind is not degraded, and quite callous to every ingenuous and noble ſentiment: but the farther we purſue our inquiries reſpecting the conſtitution of the univerſe, and the more attentively we examine the effects produced by the various modifications of the active powers which we perceive, the more we ſhall be diſpoſed to admire, adore, and love that great Firſt Cauſe which brought all things into exiſtence.

Though winter and ſummer, ſpring and autumn, and all the variety of the ſeaſons, are produced in a manner at the ſame time the moſt ſimple and the moſt ſtupendous (by the inclination of the axis of the earth to the plane of the ecliptic); yet this mechanical contrivance alone would not have been ſufficient (as I ſhall endeavour to ſhow) to produce that gradual change of temperature in the various climates which we find to exiſt, and which doubtleſs is indiſpenſably neceſſary to the preſervation of animal and vegetable life.

Though change of temperature ſeems neceſſary to the growth and perfection of moſt vegetables, yet theſe changes muſt be within certain limits. Some plants can ſupport greater changes of temperature than others, but the *extremes* of Heat and of Cold are alike fatal to all.

As the rays of the ſun are the immediate cauſe of the Heat on the ſurface of the globe, and as the length of the days in high latitudes is ſo very different in ſummer and in winter, it is evident that, in order to render thoſe regions habitable, ſome contrivance was neceſſary to prevent the conſequences which this great inequality of the Heat generated by the ſun in ſummer and in winter would naturally tend to produce; or, in other words, *to equalize the Heat*, and moderate its extremes in theſe two ſeaſons.

Let us ſee how far *Water* is concerned in this operation, and then let us examine how far the remarkable law which has been found to obtain in its condenſation by cold, tends to render it well adapted to anſwer that moſt important purpoſe.

The vaſt extent of the ocean, and its great depth,—but ſtill more its numerous currents, and the power of water to abſorb a vaſt quantity of Heat, render it peculiarly well adapted to ſerve as an equalizer of Heat.

On the retreat of the ſun after the ſolſtice, it is cloſely followed by the cold winds from the regions of eternal froſt, which are continually endeavouring to preſs in towards the equator. As the power of the ſun to warm the ſurface of the earth and the air diminiſhes very faſt in high latitudes on the days growing ſhorter, it ſoon becomes too weak to keep back the denſe atmoſphere which preſſes on from the polar regions, and the cold increaſes very faſt.

There is, however, a circumſtance by which theſe rapid advances of winter are in ſome meaſure moderated. The earth, but more eſpecially the *water*, having imbibed a vaſt quantity of Heat during the long ſummer days, while they received the influence of the ſun's vivifying beams; this Heat, being given off to the cold air which ruſhes in from the polar region, ſerves to warm it and ſoften it, and conſequently to diminiſh the impetuoſity of its motion, and take off the keenneſs of its blaſt. But as the cold air ſtill continues to flow in as the ſun retires, the accumulated Heat of ſummer is ſoon exhauſted; and all ſolid and fluid bodies are reduced to the temperature of freezing water. In this ſtage the cold in the atmoſphere increaſes very faſt, and would probably increaſe ſtill faſter, were it not for the vaſt quantity of Heat which is communicated to the air by the watery vapours, which are firſt condenſed, and then congealed, in the atmoſphere, and which afterwards fall upon the earth in the form of ſnow; and by that ſtill larger quantity which is given off by the water in the rivers and lakes, and in the ground, upon its being frozen.

But in very cold countries, the ground is frozen and covered with ſnow, and all the lakes and rivers are frozen over in the very beginning of winter. The cold then firſt begins to be extreme, and there appears to be no ſource of Heat left, which is ſufficient to moderate it in any ſenſible degree.

Let us ſee what muſt have happened if things had been left to what might be called *their natural courſe*;—if the condenſation of water on being deprived of its Heat had followed the law which we find obtains in other fluids, and even in water itſelf in ſome caſes; namely, when it is mixed with certain bodies.

Had not Providence interfered on this occaſion in a manner which may well be conſidered as *miraculous*,—all the freſh water within the polar circle muſt inevitably have been frozen to a very great depth in one winter, and every plant and tree deſtroyed; and it is more than probable, that the regions of eternal froſt would have ſpread on every ſide from the poles, and, advancing towards the equator, would have extended its dreary and ſolitary reign over a great part of what are now the moſt fertile and moſt inhabited climates of the world!

In latitudes where now the return of ſpring is hailed by the voice of gladneſs,—where the earth decks herſelf in her gayeſt attire; and millions of living beings pour forth their ſongs of joy and gladneſs, nothing would have been heard but the whiſtling of the rude winds,—and nothing ſeen but ice and ſnow, and flying clouds charged with wintry tempeſts.

Let us, with becoming diffidence and awe, endeavour to ſee what the means are which have been employed by an almighty and benevolent God to protect his fair creation.

As nouriſhment, and life, are conveyed to all living creatures through the medium of water; —*liquid*,—*living* water;—to preſerve life, it was abſolutely neceſſary to preſerve a great quantity of water in a fluid ſtate, in winter as well as in ſummer.

But in cold climates the temperature of the atmoſphere, during many months in the year, is ſo much below the freezing point, that, had not meaſures been taken to prevent ſo fatal an accident, all the water muſt inevitably have been changed to ice, which would infallibly have cauſed the deſtruction of every living thing.

Extraordinary meaſures were therefore neceſſary for preſerving in a liquid ſtate as much of the water exiſting in thoſe climates as is indeſpenſably neceſſary for the preſervation of vegetable and animal life; and this could only be done by contriving matters ſo as to prevent this water from parting with its Heat to the cold atmoſphere.

It has been ſhown,—I believe I may venture to ſay, *proved*,—in the moſt ſatisfactory manner,—

that liquids part with their Heat ONLY in conſequence of their internal motions;—and that the more rapid theſe motions are, the more rapid is the communication of the Heat;—that theſe motions are produced by the change in the ſpecific gravity of the liquid, occaſioned by the change of temperature,—and of courſe that they are more rapid, as the ſpecific gravity of the liquid is the more changed by any given change of temperature.

But it has been ſhown that the change in the ſpecific gravity of water is extremely ſmall, which takes place in any given change of temperature, *below the mean temperature of the atmoſphere;* and particularly when the temperature of the water is very near the freezing point; and hence it follows, that water muſt give off its Heat *very ſlowly* when it is near freezing.

But this is not all. There is a ſtill more extraordinary, and in its conſequences more wonderful, circumſtance which remains to be noticed. When water is cooled to within eight or nine degrees of the freezing point, it not only ceaſes to be farther condenſed, but is actually *expanded* by farther diminutions of its Heat; and this expanſion goes on as the Heat is diminiſhed, as long as the water can be kept fluid; and when it is changed to ice, it expands even ſtill more, *and the ice floats on the ſurface of the uncongealed part of the Fluid.*

Let us ſee how very powerfully this wonderful contrivance tends to retard the cooling of water when it is expoſed in a cold atmoſphere.

It is well known that there is no communication of Heat between two bodies as long as they are both at the ſame temperature; and it is likewiſe known that the *tendency* of Heat to paſs from a hot body into one which is colder, with which it is in contact, is greater, as the difference is greater in the temperatures of the two bodies.

Suppoſe now that a maſs of very cold air repoſes on the quiet ſurface of a large lake of freſh water, at the temperature of 55° of Fahrenheit's thermometer. The particles of water at the ſurface, on giving off a part of their Heat to the cold air with which they are in contact, and in conſequence of this loſs of Heat becoming ſpecifically heavier than thoſe hotter particles on which they repoſe, muſt of courſe deſcend. This deſcent of the particles which have been cooled neceſſarily forces other hotter particles to the ſurface, and theſe being cooled in their turns bend their courſe downwards; and the whole maſs of water is put into motion, and continues in motion as long as the proceſs of cooling goes on.

Before I proceed to trace this operation through all its various ſtages, I muſt endeavour to remove an objection which may perhaps be made to my explanation of this phænomenon. As I have ſuppoſed the maſs of air which reſts on the ſurface of the water to be *very cold*, and as I have taken it for granted that there is no communication whatever of Heat between the particles of water in contact with this very cold air, and the neighbouring warmer particles of water, it may be aſked, how it happens that theſe particles at the ſurface are not ſo much cooled as to be immediately changed to ice? To this I anſwer, that there are two cauſes which conſpire to prevent the *immediate* formation of ice at the ſurface of the water: *Firſt*, the ſpecific gravity of the particle of water at the ſurface being increaſed at the ſame moment when it parts with Heat, *it begins to deſcend* as ſoon as it begins to be cooled; and before the air has had time to rob it of all its Heat, *it eſcapes* and gets out of its reach: And *ſecondly*, air being a bad conductor of Heat, it cannot receive and tranſmit or *tranſport it* with ſufficient celerity to cool the ſurface of water ſo ſuddenly as to embarraſs the motions of the particles of that liquid in the operation of giving it off.

But to return to our lake: As ſoon as the water in cooling has arrived at the temperature of about 40°, as at that temperature it ceaſes to be farther condenſed, its internal motion ceaſes; and thoſe of its particles which happen to be at its ſurface remain there; and after being cooled down to the freezing point, they give off their latent Heat, and ice begins to be formed.

As ſoon as the ſurface of the water is covered with ice, the communication of Heat from the water to the atmoſphere is rendered extremely ſlow and difficult; for ice being *a bad conductor of Heat* forms a very warm covering to the water,—and moreover it prevents the water from being agitated by the wind. Farther, as the temperature of the ice at its lower ſurface is always very nearly the ſame as that of the particles of liquid water with which it is in contact, (the warmer particles of this Fluid, in conſequence of their greater ſpecific gravity, taking their places below,) the communication of Heat between the water and the ice is neceſſarily very ſlow on that account.

As ſoon as the upper ſurface of the ice is covered with ſnow, (which commonly happens ſoon after the ice is formed) this is an additional, and very powerful obſtacle to prevent the eſcape of the Heat out of the water: and though the moſt intenſe cold may reign in the atmoſphere, the increaſe of the thickneſs of the ice will be very ſlow.

During this time, the maſs of water which remains unfrozen will loſe *no part of its Heat;* on the contrary, it will continually be receiving Heat from the ground. This Heat, which is accumulated in the earth during the ſummer, will not only ſerve, in ſome meaſure, to replace that which is communicated to the atmoſphere through the ice, and prevent its being furniſhed at the expence of the latent Heat of the water in contact with its ſurface, but, when the temperature of the air is not much below that of freezing, this ſupply of Heat from below will be quite ſufficient to replace that which the air carries off; and the thickneſs of the ice will not increaſe.

Whenever the temperature of the air is not actually *colder* than freezing water, the Heat which riſes from the bottom of the lake will be all employed in melting the ice at its under ſurface, and diminiſhing its thickneſs.

ed happen, when the ice is very
cially when its upper ſurface is
ep ſnow, that the melting of the
rface may be going on, when the
ure of the atmoſphere is conſiderably below the freezing point.

As the particles of water which, receiving Heat from the ground at the bottom of the lake, acquire a higher temperature than that of 40°, and being *expanded*, and becoming ſpecifically lighter by this additional Heat, riſe up to the upper ſurface of the fluid water, and give off their ſenſible Heat to the under ſurface of the ice, never return to the bottom, this communication of the Heat which exhales from the earth produces very little motion in the maſs of the water; and this circumſtance is, no doubt, very favourable to the preſervation of the Heat of the water.

When a ſtrong wind prevails, and the ſurface of the water is much agitated, ice is not formed, even though the whole maſs of water ſhould, by a long continuance of cold weather, have been previouſly cooled down to that point to which it is neceſſary that it ſhould be brought, in order that its internal motions may ceaſe, and it may be diſpoſed to congeal; for though the particles at and near the ſurface may no longer have any tendency to deſcend, on being farther cooled, yet, as they have ſo conſiderable a quantity of ſenſible Heat (eight or ten degrees) to diſpoſe of, after their condenſation with cold ceaſes, and as the agitation into which the water is thrown by the wind does not permit any particle to remain long enough in contact with the cold air to give off all its Heat at once, there is a continual ſucceſſion of freſh particles at the ſurface, all of which give off Heat to the air; but none of them have time to be cooled ſufficiently to be diſpoſed to form ice. The water will loſe a vaſt quantity of Heat, and as ſoon as the wind ceaſes, if the cold ſhould continue, ice will be formed very rapidly.

But it is not merely the agitation of the water which renders the communication of the Heat very rapid, the agitation of the wind alſo tends to produce the ſame effect.

On the return of ſpring, the ſnow melting before the ſun as he advances and his rays become more powerful, all the Heat which the earth exhales is employed in diſſolving the ice at its under ſurface, while the ſun on the other ſide acts ſtill more powerfully to produce the ſame effect.

Though ice is tranſparent, yet it is not perfectly ſo; and as the light which is ſtopped in its paſſage through it cannot fail to generate Heat *when* and *where* it is ſtopped, or abſorbed, it is by no means ſurpriſing that ſnow ſhould be found to melt when expoſed in the ſun's rays, even when the temperature of the air in the ſhade is conſiderably below the point of freezing. Snow expoſed to the ſun melts long before the even ſurface of ice begins to be ſenſibly ſoftened by its beams, and it is not till ſome time after all the hills are bare that the ice on the lakes and rivers breaks up.

The rays which penetrate a bank of ſnow being often reflected and refracted, deſcend deep into it, and the Heat is depoſited in a place where it is not expoſed to be carried off by the cold air of the atmoſphere; but the rays which fall upon the horizontal and ſmooth ſurface of ice, are moſtly reflected upwards into the atmoſphere; and if any part of them are ſtopped at the ſurface of the ice, the Heat generated by them *there* is inſtantaneouſly carried off by the cold air, and a particle of water is no ſooner made fluid than it is again frozen.

Hence we ſee that the ſnow which in cold countries covers the ice that is formed on the ſurface of freſh water, not only prevents the Heat of the water from being carried off by the air, during the winter, but alſo aſſiſts very powerfully in thawing the ice early in the ſpring.

Should the waters of a lake be ſo deep, or ſo imperfectly tranſparent, as to intercept a great proportion of rays of the ſun before they reach the bottom, in that caſe, the temperature of the water at the bottom of the lake will be *nearly the ſame all the year round;* and in countries where there is *any* froſt in winter, and particularly in thoſe lakes which lie near high mountains, and are fed by torrents which proceed from *Glaciers*, and melting ſnow, this *conſtant temperature* at the bottom can never be much above or below 41° F. whatever may be the heat to which the *ſurface* of the lake is expoſed in ſummer, or however long and intenſely hot the ſummer may be*.

Let us now ſee what the conſequences would have been, had the condenſation of water with cold followed the law which obtains in regard to all other Fluids.

As the internal motion of the water could not have failed to continue as long as its ſpecific gravity continued to be increaſed by parting with Heat, ice would not have begun to be formed till the whole maſs of water had arrived at the temperature of 32° of Fahrenheit's thermometer.

To

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* In a letter from Profeſſor PICTET of Geneva to the Author, of the 7th July 1797, accompanying the 36th number of the BIBLIOTHEQUE BRITANNIQUE, (in which an account, or rather tranſlation, of the firſt Edition of this Eſſay is publiſhed in the French language,) there is the following paragraph.

"I took the liberty to throw in, as uſual," (in the tranſlation) "ſome occaſional notes; one of which will, I hope, deſerve your attention. It points out the near coincidence of the mean temperature of the bottom, obſerved in ten different lakes, by M. de Sauſſure and myſelf, viz. 4⅓° R."—(equal to 41¾° F.) "with the temperature where the *minimum* of volume, or *maximum* of denſity of water takes place. We vainly ſtrove, to this day, to explain the uniformity we obſerved in that particular in ſeveral lakes very differently ſituated, in many reſpects, but your reflections ſeem to me fully to reſolve the problem."

The following is the note in the *Bibliothèque Britannique,* alluded to by Profeſſor PICTET, in the foregoing paragraph of his letter.

"Ce n'eſt pas ſeulement dans le lac de Genève que M. de Sauſſure, "notre ſavant ami, a fait les expériences curieuſes qui ſont ici rappellées, et à quelques-unes des quelles nous avons eu le plaiſir d'aſſiſter; il les a répétées dans la Méditerranée, et dans dix lacs qui bordent de part et d'autre la chaine des Alpes. Nous

To ſee what an enormous quantity of Heat would be loſt, when the water is deep, in conſequence of its whole maſs being cooled in this manner, we have only to compute how much ice this Heat would melt; or how much water it would heat from the point of freezing to that of boiling.

It has been ſhown by experiment, that any given quantity of ice requires as much Heat to melt it as an equal quantity of fluid water loſes in cooling

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" tirons de ſon grand ouvrage ſur les montagnes les températures " obſervées au fond de ces lacs comme ſuit;

" Noms des Lacs.	Profondeurs en pieds de France.	Températures du fond Degrés de Reaumur.
" Lac de Genève	950	4.3
" de Neuchâtel	325	4.1
" de Bienne	217	5.5
" du Bourget	240	4.5
" d'Annecy	163	4.5
" de Thun	350	4.0
" de Brientz	500	3.8
" de Lucerne	600	3.9
" de Conſtançe	370	3.4
" Lac Majeur	335	5.4

" Température moyenne du fond de dix lacs 4.34, ou $4\frac{1}{3}$° R."

" Il n'eſt peutêtre aucun de nos lecteurs qui, plein des idées que " notre auteur vient de diſcuter, ne ſoit frappé de la coincidence entre " cette température du fond des lacs dans nos latitudes moyennes et " celle à laquelle l'eau atteint ſon *minimum* de volume ou *maximum* de " denſité! La permanence de cette température, et ſon identité dans " des lacs d'ailleurs très-diverſement ſitués, paroiſſent intimément " liées avec cette circonſtance du *minimum* de volume. Mais ce n'eſt " pas ici le lieu de donner cours aux idées que peut ſuggerer ce " rapprochement; nous l'indiquons à l'auteur comme un objet digne " de ſes méditations."

The Author of this Eſſay feels himſelf very much obliged to his ingenious and reſpectable friend, Profeſſor PICTET, for theſe intereſting obſervations.

140 degrees; conſequently the quantity of ice which might be melted by the Heat given off by any given quantity of water in cooling any given number of degrees, is to the given quantity of water, as the number of degrees which it is cooled, to 140 degrees.

Hence it follows that when the temperature of the water is 8 degrees above the freezing point, it gives off in cooling down to that temperature as much Heat as would melt $\frac{8}{140}$ or $\frac{2}{35}$ of its weight of ice; the water, therefore, which is cooled from the temperature of 40° to that of 32°, if it be 35 feet deep, will give off as much Heat in being ſo cooled as would melt a covering of ice 2 feet thick.

But this even is not all: for as the particles of water, on being cooled at the ſurface, would, in conſequence of the increaſe of their ſpecific gravity on parting with a portion of their Heat, immediately deſcend to the bottom, the greateſt part of the Heat accumulated during the ſummer in the earth on which the water repoſes would be carried off and loſt, before the water began to freeze; and when ice was once formed, its thickneſs would increaſe with great rapidity, and would continue increaſing during the whole winter; and it ſeems very probable that, in climates which are now temperate, the water in the large lakes would be frozen to ſuch a depth in the courſe of a ſevere winter that the Heat of the enſuing ſummer would not be ſufficient to thaw them; and ſhould this once happen, the following winter could hardly fail to change the whole maſs of its waters to one ſolid body of ice, which never more could recover its liquid form, but muſt remain immovable till the end of time.

In the month of February, after a froſt which had laſted a month, the temperature of the air being 38°, M. DE SAUSSURE found the temperature of the water of the Lake of Geneva, at the ſurface, at 41°,—and at the depth of 1000 feet, at 40°. Had the froſt continued but a little longer, ice would have been formed; but had the conſtitution of water been ſuch that the whole maſs of that Fluid in the Lake muſt have been cooled down to the temperature of 32° before ice could have been formed, this event could not have happened till the water had given off as much Heat as would be ſufficient to melt a covering of ice above 57 feet thick!—

This quantity of Heat would be ſufficient to heat, to the point of boiling, a quantity of ice-cold water as large as the Lake, and 49 feet deep!

We cannot ſufficiently admire the ſimplicity of the contrivance by which all this Heat is ſaved. It well deſerves to be compared with that by which the ſeaſons are produced; and I muſt think that every candid inquirer, who will begin by diveſting himſelf of all unreaſonable prejudices, will agree with me in attributing them both TO THE SAME AUTHOR.

When we trace ſtill farther the aſtoniſhing effects that are produced in the world by the operations of that ſimple law which has been found to obtain in the condenſation of water on its being deprived of Heat, we ſhall find more and more reaſon to admire the wiſdom of the contrivance.

That high latitudes might be habitable, it was neceſſary that vegetables ſhould be protected from the effects of the chilling froſts of a long and ſevere winter: but if it be true that watery liquids do not part with their Heat but *in conſequence of their internal motions;* and if theſe motions are occaſioned *merely* by the change produced in the ſpecific gravity of thoſe particles of the liquid which receive Heat, or which part with it, who does not ſee how very powerfully the ſudden diminution and final ceſſation of the condenſation of water in cooling, as ſoon as its temperature approaches to the freezing point, operates to prevent the ſap in vegetables from being frozen?

But if, for the purpoſes of life and vegetation, it be neceſſary that the ground, the rivers, the lakes, and the trees be defended from the cold winds from the poles, it may be aſked how this inundation of cold air is to be warmed?—I anſwer, by the waters of the ocean; which there is the greateſt reaſon to think, were not only deſigned principally for that uſe, but particularly *prepared* for it.

Sea-water contains a large proportion of ſalt in ſolution; and we have ſeen that the condenſation of a ſaline ſolution, on its being cooled, follows a law which is extremely different from that obſerved in regard to pure water; and which (as may eaſily be ſhown) renders it peculiarly well adapted for communicating Heat to the cold winds which blow over its ſurface.

As ſea water continues to be condenſed as it goes on to cool, even after it has paſſed the point at which freſh water freezes, the particles at the ſurface, inſtead of remaining there after the maſs of the water had been cooled to about 40°, and preventing the other warmer particles below from coming in their turns and giving off their Heat to the cold air, (as we have ſeen always happens when freſh, or *pure water* is ſo cooled,) theſe cooled particles of *ſalt water* deſcend as ſoon as they have parted with their Heat, and in moving downward force other *warmer* particles to move upwards; and in conſequence of this continual ſucceſſion of warm particles, which come to the ſurface of the ſea, a vaſt deal of Heat is communicated to the air;—incomparably more than could poſſibly be communicated to it by an equal quantity of freſh water, at the ſame temperature, as will appear by the following computation,

Without taking into the account that very great advantage which ſea water poſſeſſes over freſh water, conſidered as an equalizer of the temperature of the atmoſphere, which ariſes from the comparative *lowneſs of the point of its congelation*;—ſuppoſing even ſea water to freeze at as high a temperature as freſh water, namely, at 32°; and ſuppoſing (what is ſtrictly true) that as ſoon as either ſea water or freſh water is frozen at its ſurface, and this ice is covered with ſnow, the communication of Heat from the water to the atmoſphere ceaſes almoſt entirely;—we will endeavour to determine how much more Heat would, even on this ſuppoſition, be communicated to the air by ſalt water than by freſh water, after both have arrived at the temperature of 40°.

When freſh water, in cooling, has arrived at this temperature, it ceaſes to be farther condenſed with cold, and its internal motions (which, as we have already more than once obſerved, are cauſed *ſolely* by the changes produced in the ſpecific gravity of its particles) ceaſe of courſe, and ice immediately begins to be formed on its ſurface; but as the condenſation of ſalt water goes on as its Heat goes on to be diminiſhed, its internal motions will continue; and it is evidently impoſſible for ice to be formed at its ſurface till the whole maſs of the water has become ice-cold, or till its temperature is brought down to 32°. It would therefore give off a quantity of Heat equal to 8 degrees, at leaſt, of Fahrenheit's thermometer, *more than the freſh water* would part with before ice could be formed on its ſurface.

To be able to form an idea of this enormous quantity of Heat, we have only to recollect what has already been ſaid, and we ſhall find reaſon to conclude that it would be ſufficient to melt a covering of ice equal in thickneſs to $\frac{2}{35}$ of the depth of the ſea.—It would therefore be ſufficient, in that part of the North Sea (lat. 67°) where Lord Mulgrave ſounded at the depth of 4680 feet, to melt a cake of ice 265 feet thick!

But the Heat evolved in the formation of each ſuperficial foot of ice would be ſufficient to raiſe the temperature of a ſtratum of incumbent air 2220 times as thick as the ice, (conſequently in the caſe in queſtion 265×2220 feet, or 869 miles thick,) 18 degrees, or from the temperature of freezing water, to that of 50° of Fahrenheit's thermometer, or to the mean annual temperature of the northern parts of Germany!

The Heat given off to the air by each ſuperficial foot of water, in cooling *one degree*, is ſufficient to heat an incumbent ſtratum of air 44 times as thick as the depth of the water, 10 degrees. Hence we ſee how very powerfully the water of the ocean, which is never frozen over, except in very high latitudes, muſt contribute to warm the cold air which flows in from the polar regions.

But the ocean is not more uſeful in moderating the extreme cold of the polar regions, than it is in tempering the exceſſive heats of the torrid zone;—and what is very remarkable, the fitneſs of the ſea water to ſerve this laſt important purpoſe is owing to the very ſame cauſe which renders it ſo peculiarly well adapted for communicating Heat to the cold atmoſphere in high latitudes, namely, *to the ſalt which it holds in ſolution.*

As the condenſation of ſalt water, with cold, continues to go on even long after it has been cooled to the temperature at which freſh water freezes, thoſe particles at the ſurface which are cooled by an immediate contact with the cold winds muſt deſcend, and take their places at the bottom of the ſea, where they muſt remain, till, by acquiring an additional quantity of Heat, their ſpecific gravity is again diminiſhed. But this Heat *they never can regain in the polar regions;* for innumerable experiments have proved, beyond all poſſibility of doubt, that there is no *principle of Heat* in the *interior parts of the globe*, which, by exhaling through the bottom of the ocean, could communicate Heat to the water which reſts upon it.

It has been found that the temperature of the earth at great depth under the ſurface is different in different latitudes, and there is no doubt but this is alſo the caſe with reſpect to the temperature at the bottom of the ſea, in as far as it is not influenced by the currents which flow over it; and this proves, to a demonſtration, that the Heat which we find to exiſt, without any ſenſible change during ſummer and winter, at great depths, is owing to the action of the ſun, and not to *central fires*, as ſome have too haſtily concluded.

But if the water of the ocean, which, on being deprived of a great part of its Heat by cold winds, deſcends to the bottom of the ſea, cannot be warmed *where it deſcends*, as its ſpecific gravity is greater than that of water at the ſame depth in warmer latitudes, it will immediately begin to ſpread on the bottom of the ſea, and to flow towards the equator, and this muſt neceſſarily produce a current at the ſurface in an oppoſite direction; and there are the moſt indubitable proofs of the exiſtence of both theſe currents.

The proof of the exiſtence of one of them would indeed have been quite ſufficient to have proved the exiſtence of both, for one of them could not poſſibly exiſt without the other; but there are ſeveral direct proofs of the exiſtence of each of them.

What has been called the gulph ſtream, in the Atlantic Ocean, is no other than one of theſe currents, that at the ſurface, which moves from the equator towards the north pole, modified by the trade winds and by the form of the continent of North America; and the progreſs of the lower current may be conſidered as proved directly by the cold which has been found to exiſt in the ſea at great depths in warm latitudes;—a degree of temperature much below the mean annual temperature of the earth in the latitudes where it has been found, and which of courſe muſt have been *brought from colder latitudes.*

The mean annual temperature in the latitude of 67° has been determined by Mr. KIRWAN, in his excellent treatiſe on the temperature of different latitudes, to be 39°; but Lord Mulgrave found, on the 20th of June, when the temperature of the air was 48½°, that the temperature of the ſea at the depth of 4680 feet in that latitude was 6 degrees below freezing, or 26° of Fahrenheit's thermometer.

On the 31ſt of Auguſt, in the latitude of 69°, where the annual temperature is about 38°, the temperature of the ſea at the depth of 4038 feet was 32°; the temperature of the atmoſphere (and probably that of the water at the ſurface of the ſea) being at the ſame time at 59½°.

But a ſtill more ſtriking, and I might, I believe, ſay, an incontrovertible proof of the exiſtence of currents of cold water at the bottom of the ſea, ſetting from the poles towards the equator, is the very remarkable difference that has been found to ſubſiſt between the temperature of the ſea at the ſurface and at great depth, at the tropic,—though the temperature of the atmoſphere *there* is ſo conſtant that the greateſt changes produced in it by the ſeaſons ſeldom amounts to more than five or ſix degrees; yet the difference between the Heat of the water at the ſurface of the ſea, and that at the depth of 3600 feet, has been found to amount to no leſs than 31 degrees; the temperature above or at the ſurface being 84°, and at the given depth below no more than 53°*.

It appears to me to be extremely difficult, if not quite impoſſible, to account for this degree of cold at the bottom of the ſea in the torrid zone, on any other ſuppoſition than that of *cold currents from the poles;* and the utility of theſe currents in tempering the exceſſive heats of thoſe climates is too evident to require any illuſtration.

Theſe currents are produced, as we have already ſeen, in conſequence of the difference in the ſpecific gravity of the ſea water at different temperatures; their velocities muſt therefore be in proportion to the change produced in the ſpecific gravity of water by any given change of temperature; and hence we ſee how much greater they muſt be in *ſalt water* than they could poſſibly have been had the ocean been compoſed of freſh water.

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* Phil. Tranſactions, 1752.

It is not a little remarkable that the water of all great lakes is freſh, and nearly ſo in all inland ſeas (like the Baltic) in cold climates, and which communicate with the ocean by narrow channels. We ſhall find reaſon to conclude that this did not happen without deſign, when we conſider what conſequences would probably enſue ſhould the waters of a large lake in an inland ſituation, in a cold country, (ſuch as the lake Superior, for inſtance, in North America,) become as ſalt as the ſea.

Though the cold winds which blow over the lake in the beginning of winter would be more warmed, and the temperature of the air on the ſide of the lake oppoſite to the quarter from whence theſe winds arrive, would be rendered ſomewhat milder than it now is; yet, as the water of the lake would give off an immenſe quantity of Heat before a covering of ice could be formed on its ſurface for its protection, it would, on the return of ſpring, be found to be *extremely cold;* and as it would require a long time to regain from the influence of the returning ſun the enormous quantity of Heat loſt during the winter, it would remain very cold during the ſpring, and probably during the greateſt part of the ſummer; and this could not fail to chill the atmoſphere, and check vegetation in the ſurrounding country to a very conſiderable diſtance. And though a large lake of ſalt water in a cold country would tend to render the winter *ſomewhat milder* on one ſide of it, namely, on the ſide oppoſite to the quarter from whence the cold winds came; yet this advantage would not only be confined to a ſmall tract of country, but would not any where be very important, and would by no means counterbalance the extenſive and fatal conſequences which would be produced in ſummer by ſo large a collection of very cold water.

When the winter is once fairly ſet in,—when the earth is well covered with ſnow, and the rivers and lakes with ice, and more eſpecially when the ice as well as the land is covered with that warm winter garment, a few degrees more of cold in the air cannot produce any laſting bad conſequences. It may oblige the inhabitants to uſe additional precautions to guard themſelves, their domeſtic animals, and their proviſions, from the uncommon ſeverity of the weather; but it can have very little influence in the temperature of the enſuing ſummer; and even it is probable, if it influences it at all, that it tends rather to make it *warmer* than *colder*. Lakes of ſalt water could therefore be of no real uſe *in winter* in cold countries, and in ſummer they could not fail to be very hurtful; while freſh lakes, as they are frozen over almoſt as ſoon as the winter ſets in, and long before the whole maſs of their water is cooled down to the temperature of freezing, preſerve the greater part of their Heat through the winter, and if they are of no conſiderable uſe during the cold ſeaſon, they probably do little or no harm in ſummer.

But I muſt take care not to tire my reader by purſuing theſe ſpeculations too far. If I have perſiſted in them,—if I have dwelt on them with peculiar ſatisfaction and complacency,—it is becauſe I think them uncommonly intereſting,—and alſo becauſe I conceived that they might be of real uſe in this age of *refinement* and *ſcepticiſm*.

If, among barbarous nations, the *fear of a God*, and the practice of religious duties, tend to ſoften ſavage diſpoſitions, and to prepare the mind for all thoſe ſweet enjoyments which reſult from peace, order, induſtry, and friendly intercourſe,—a *belief in the exiſtence of a Supreme Intelligence*, who rules and governs the univerſe with wiſdom and goodneſs, is not leſs eſſential to the happineſs of thoſe, who, by cultivating their mental powers, HAVE LEARNED TO KNOW HOW LITTLE CAN BE KNOWN.

Fig. 1

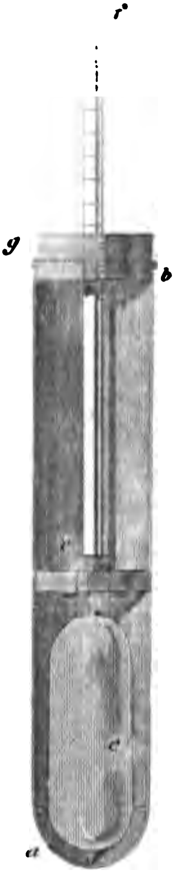
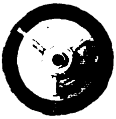
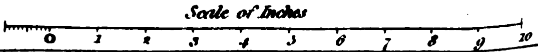


Fig. 2





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DESCRIPTION OF THE PLATES.

PLATE I.

THIS Plate repreſents the cylindrical Paſſage Thermometer uſed in the Experiments, on the conducting power of liquids with regard to Heat.

Fig. 1. *a b.* is a ſection of the braſs tube in which the Thermometer *c*, with an oblong copper bulb, is placed.

e f is the glaſs tube of the Thermometer, which, for want of room in the Plate, is repreſented as broken off at *f*.

g is a ſtopple of cork by which the end of the braſs tube, *a b*, is cloſed; and

h is a circular diſk of the ſame ſubſtance.

The ſpace in the braſs tube below this diſk *h*, ſurrounding the bulb of the Thermometer, was occupied by the liquid whoſe conducting power was determined. The ſpace between the diſk and the cork-ſtopper *g*, was filled with eider-down.

Between the inſide of the braſs tube and the lower part of the bulb of the Thermometer are ſeen the wooden pins which ſerved to confine the Thermometer in its place.

Fig. 2. This is an horizontal ſection of the braſs tube, and a bird's eye view of the Thermometer in its place.

PLATE II.

Fig. 3. This Figure ſhows the manner in which the Experiments were made, in which a cake of ice at the bottom of a tall glaſs jar was thawed by hot water ſtanding on its ſurface.

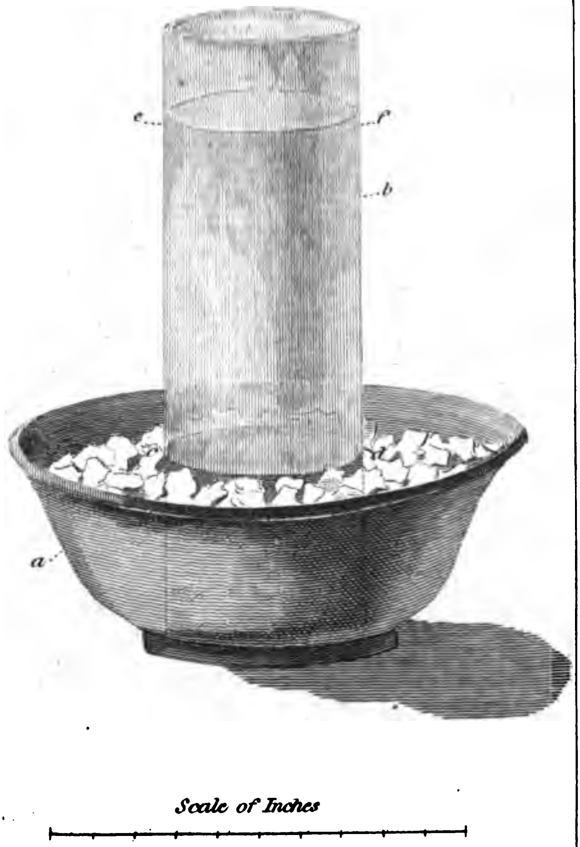
a is an earthen bowl filled with pounded ice and water, in which the glaſs jar, *b*, was placed.

c d is the level of the upper ſurface of the ice in the jar.

e f is the level of the ſurface of the water ſtanding on the ice in the jar.

END OF PART I. OF ESSAY VII.

Fig. 3



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ESSAY VII.

PART II.

An Account of ſeveral NEW EXPERIMENTS, with occaſional Remarks and Obſervations, and CONJECTURES reſpecting Chemical Affinity, and Solution; and the mechanical Principle of Animal Life.

CHAP. I.

*Account of a Circumſtance of a private Nature, by which the Author has been induced to add this and the following Chapters to the Second Edition of this Eſſay.—Experimental Inveſtigation of the Subject continued.—*OIL *found by Experiment to be a Non-conductor of Heat.—*MERCURY *is likewiſe a Non-conductor.—Probability that all* FLUIDS *are* NON-CONDUCTORS, *and that this Property is* ESSENTIAL TO FLUIDITY.*—The Knowledge of that Fact may be of great Uſe in enabling us to form more juſt Ideas with regard to the Nature of thoſe mechanical Operations which take place in chemical Solutions and Combinations; in the Proceſs of Vegetation; and in the various Changes effected by the Powers of Life in the Animal Economy.—Rapidity of Solution no Proof of the Exiſtence of an*

Attraction of Affinity.—Strata of freſh Water and of ſalt Water may be made to repoſe on each other in actual Contact without mixing.—Probability that the Water at the Bottom of freſh Lakes, that are very deep, may be actually ſalt.

AT the end of a French tranſlation of the Firſt Edition of this Eſſay, publiſhed at Geneva, Profeſſor Pictet (the tranſlator) has added the following extract of one of my private letters to him, (of the 9th June 1797,) written in anſwer to one from him to me, acknowledging the receipt of a manuſcript copy of the Eſſay which I had ſent him.

" I ſhould have been much ſurpriſed if my " Seventh Eſſay had not intereſted you; for in my " life I never felt pleaſure equal to that I enjoyed " in making the experiments of which I have given " an account in that performance. You will per- " haps be ſurpriſed when I tell you, that I have " ſuppreſſed a whole Chapter of intereſting ſpecu- " lation, merely with a view of leaving to others a " tempting field of curious inveſtigation *untouched;* " and to give more effect to my concluding reflec- " tion; which I conſider as being by far the moſt " important of any I have ever publiſhed."

As theſe aſſertions, (which were not originally intended for the public eye,)—are liable to ſeveral interpretations, I think it my duty, not only to explain them, but alſo to let the Public know preciſely how far I have puſhed my inquiries in the inveſtigation of the ſubject under conſideration: This is an act of juſtice which I owe to thoſe who may be engaged in the ſame purſuits; for it would be very unfair, by *obſcure hints of important information kept back*, to keep others in doubt with reſpect to the originality of the diſcoveries they may make in the proſecution of *their* inveſtigations. This would tend to *damp* the ſpirit of inquiry, inſtead of *exciting* it; and throwing out ſuch hints looks ſo much like lying in wait to ſeize on the fair fruits of the labours of others, that I cannot reſt till I have ſhewn that I do not deſerve to be ſuſpected of ſuch pitiful views.

My worthy friend, Profeſſor Pictet, certainly did not ſuſpect any unhandſome deſign in any thing I ſaid to him in my (private) letter; but thoſe who are leſs acquainted with my character, may not be diſpoſed to give me credit for candour and diſintereſtedneſs without proofs.

With regard to the aſſertion in my letter, "that "I had ſuppreſſed a whole Chapter of intereſting "ſpeculation, with a view to leaving to others a "tempting field, *untouched*, for curious inveſtiga"tion;"—this is perfectly true in fact, as will, I flatter myſelf, appear, by what I ſhall now lay before the Public; and I am confident that thoſe who will take the trouble to conſider with attention the reaſons which induced me to do this, will find them ſuch as will deſerve their approbation.

Having, as I flattered myſelf, laid open a new and moſt enticing proſpect to thoſe who are fond of philoſophical purſuits, I was afraid, if I advanced too far, that others, inſtead of ſtriking out roads for themſelves, might perhaps content themſelves with following in my footſteps; and conſequently that many, and probably the moſt intereſting parts of the new field of inquiry, would remain a long time unexplored: And with regard to the reputation of being a *diſcoverer*, though I rejoice, I might ſay, exult and triumph—in the progreſs of human knowledge,—and enjoy the ſweeteſt delight in contemplating the advantages to mankind which are derived from the introduction of uſeful improvements; yet I can truly ſay, that I ſet no very high value on the honour of being the firſt to ſtumble on thoſe treaſures which every where lie ſo ſlightly covered.

In reſpect to the "concluding reflection" of the Firſt Edition of this Eſſay;—though ſome may ſmile in pity; and others frown at it; I am neither aſhamed, nor afraid to own, that I conſider the ſubject as being of the *utmoſt importance* to the peace, order, and happineſs of mankind, *in our preſent advanced ſtate of ſociety*. But to return from theſe digreſſions—

Though it appeared to me that the important fact I undertook to inveſtigate, relative to the *manner* in which Heat is propagated in Fluids, is fully eſtabliſhed by the Experiments, of which an account has been given in the preceding Chapters of this Eſſay; yet, as a thorough examination of the ſubject is a matter of much importance, in many reſpects, I did not reſt my inquiries here, but made a number of Experiments with a view to throwing ſtill more light upon it, and enabling us to form more clear and diſtinct ideas reſpecting thoſe curious mechanical operations which appear to take place in Fluids, when Heat is propagated in them.

Having frequently obſerved when a quantity of water in one of my glaſs jars was frozen to a cake of ice, by placing the jar in a freezing mixture, that, as the ice firſt began to be formed at the ſides of the jar, and increaſed gradually in thickneſs, the portion of water in the axis of the jar (which laſt retained its fluidity) being compreſſed by the expanſion of the ice, was always forced upwards towards the end of the proceſs, and formed a pointed projection of ice in the form of a nipple, (*papilla,*) which was ſometimes above half an inch high in the middle of the upper ſide of the cake; I was led by that circumſtance to make the following intereſting Experiments.

Experiment, No. 55.

A cake of ice, 3 inches thick, which had a pointed projection, ½ an inch high, which aroſe from the centre of its upper ſurface, being frozen faſt in the bottom of a tall cylindrical glaſs jar, 4¾ inches in diameter; this jar, ſtanding in an earthen pan, and being ſurrounded by pounded ice and water, to the height of an inch above the level of the upper ſurface of the cake of ice, was placed on a table, near a window, in a room where the air was at the temperature of 31° of Fahrenheit's thermometer; and fine *olive oil*, which had previouſly been cooled down to the temperature of 32°, was

poured

poured into the jar till it ſtood at the height of 3 inches above the ſurface of the cake of ice.

Having ready a ſolid cylinder of wrought iron, 1¼ inch in diameter, and 12 inches long, with a ſmall hook at one end of it, by means of which it could occaſionally be ſuſpended in a vertical poſition, and furniſhed with a fit hollow cylindrical ſheath of thick paper, into which it juſt paſſed,—open at both ends, and about $\frac{1}{10}$ of an inch longer than the ſolid cylinder of iron, to which it ſerved as a covering for keeping it warm; this iron cylinder, being heated to the temperature of 210° in boiling water, and being ſuddenly introduced into its ſheath, was ſuſpended by an iron wire which deſcended from the ceiling of the room, in ſuch a manner, that its lower end entering the jar, (in the direction of its axis,) was immerſed in the oil to ſuch a depth, that the middle of the flat ſurface of this end of the hot iron, which was directly above the point of the conical projection of ice, was diſtant from it only $\frac{2}{10}$ of an inch. The end of the ſheath deſcended $\frac{1}{10}$ of an inch lower than the end of the hot metallic cylinder.

As the oil was very tranſparent, and the jar placed in a favourable light, the conical projection of ice was perfectly viſible, even after the hot cylinder was introduced into the jar; and had *any Heat* DESCENDED through the thin ſtratum of fluid oil which remained interpoſed between the hot ſurface of the iron and the pointed projection of ice which was under it, there is no doubt but this Heat muſt have been apparent, by the melting of the ice; which event would have been diſcovered, either by the diminution of the height of this projection, or by an alteration of its form. But this was not the caſe: the ice did not appear to be in the ſmalleſt degree diminiſhed, or otherwiſe affected by the vicinity of the hot iron.

My reader will naturally ſuppoſe, without my mentioning the circumſtance, that due care was taken in introducing the cylinder into the jar, to do it in the moſt gentle manner poſſible, to prevent the oil from being thrown into undulatory motions; and that proper means were uſed for confining the cylinder, motionleſs, in its place, when it had arrived there.

As this experiment appears to me to be unexceptionable, and its reſult unequivocal and deciſive; in order that a perfect idea may the more eaſily be formed of it, I have added the Figure 4, where a ſection of the whole of the apparatus uſed in making it may be ſeen, expreſſed in a clear and diſtinct manner.

If the general reſult of the Experiments, of which an account has been given in the two firſt Chapters of this Eſſay, afforded reaſon to conclude that *water* is a *non-conductor* of Heat, the reſult of that here deſcribed certainly proves, in a manner quite as ſatisfactory, that *oil* is alſo a *non-conductor*; and ſerves to give an additional degree of probability to the conjecture, that all Fluids are *neceſſarily* non-conductors of Heat.

As *mercury*, which is a metal in fuſion, is different in many reſpects from all other Fluids, I was very impatient to know if it agreed with them in that eſſential property, from which they have been denominated non-conductors of Heat, and this I found to be actually the caſe, by the reſult of the following deciſive Experiment.

Experiment, No. 56.

Having emptied and cleaned out the cylindrical glaſs jar uſed in the laſt mentioned Experiment, and repleniſhed it with a freſh cake of ice, with a conical projection in the middle of its upper ſide, I placed the jar, ſurrounded by pounded ice and water, on the table, in the cold room, where the foregoing Experiment had been made; and poured over the cake of ice as much ice-cold *mercury*, as covered it to the height of about an inch. Having cleaned the ſurface of the mercury in the jar with blotting paper, I ſuffered the whole to remain quiet about an hour; and then very gently introduced the end of the hot cylinder of iron (incloſed in its paper ſheath) into the mercury, and fixed it immoveably in ſuch a poſition, that its flat end, which was naked, was immediately over the point of the conical projection of ice, and diſtant from it about ¼ of an inch; where I ſuffered it to remain ſeveral minutes.

It is neceſſary that I ſhould mention, that, in order to prevent the internal motions in the maſs of mercury, which would otherwiſe have been occaſioned by the riſing and ſpreading out on its ſurface of thoſe particles of that fluid, which, having touched the flat end of the hot iron, became ſpecifically lighter in conſequence of their increaſe of temperature, the end of the hollow cylindrical ſheath, in which the ſolid cylinder of iron was placed, was made to project about $\frac{1}{10}$ of an inch below the flat end of the iron. This precaution was likewiſe uſed, and for a ſimilar reaſon, in the preceding Experiment; when oil was uſed in the place of the mercury; as was mentioned, though without being explained, in giving an account of that Experiment.

As the cake of ice, on which the mercury repoſed, was at that temperature preciſely at which ice is diſpoſed to melt with the ſmalleſt additional quantity of Heat, if *any Heat* had found its way *downwards* through the mercury to the ice in this Experiment, water would moſt undoubtedly have been formed; and this water would as undoubtedly have appeared on the ſurface of the mercury on taking away the iron: but there was not the ſmalleſt appearance of any ice having been melted.

To find out whether the cake of ice was *really* at that temperature at which it was diſpoſed to melt with any additional Heat, I thruſt down the end of my finger through the mercury, and touched the ice; and this Experiment removed all my doubts; for I found that, however expeditiouſly I performed that operation, it was hardly poſſible for me to touch the ice without evident ſigns of water having been produced being left behind, on the clean and bright ſurface of the mercury, on taking away my finger.

From the reſults of all theſe experimental inveſtigations it appears to me, that we may ſafely conclude that *water*, *oil*, and *mercury* are perfect *non-conductors* of Heat; or, that when either of thoſe ſubſtances takes the form of a Fluid, all interchange and communication of Heat *among its particles*, or from one of them to the other, directly, becomes from that moment *abſolutely impoſſible.*

That this is alſo the caſe with reſpect to the particles of *air*, has been rendered extremely probable, —I believe I might ſay proved,—by the Experiments of which I gave an account in one of my Papers on Heat, publiſhed in the Tranſactions of the Royal Society;—and I have ſhewn elſewhere—(in my Sixth Eſſay) how much reaſon there is to conclude, that the particles of *Steam* and of *Flame* are in the ſame predicament.

But if all interchange and communication of Heat, from particle to particle—*immediately*, or *de proche en proche*, be abſolutely impoſſible in ſo many *elaſtic* and *unelaſtic Fluids*,—and in Fluids ſo eſſentially different in many other reſpects,—is there not ſufficient grounds to conclude from hence, that this property is common to all Fluids—and that it is even *eſſential to fluidity?*

It is eaſy to conceive, that the diſcovery of ſo important a circumſtance muſt neceſſarily occaſion a conſiderable change in the ideas we have formed in reſpect to the mechanical operations which take place in many of the great phenomena of Nature; as well as in many of thoſe ſtill more intereſting chemical operations, which we are able to direct, but which we find, alas! very difficult to explain.

In my Paper on Heat, above mentioned, publiſhed in the Philoſophical Tranſactions for the year 1792, I endeavoured to apply the diſcovery of the non-conducting power of *air* in accounting for the warmth of the hair of beaſts;—of the feathers of birds;—of artificial clothing;—and of ſnow, *the winter garment of the earth;*—and alſo, in explaining the cauſes of the cold winds from the polar regions, and of their different directions in different countries, which prevail at the end of winter, and early in the ſpring.

In my Sixth Eſſay—(on the Management of Heat and the Economy of Fuel)—I availed myſelf of the knowledge of the non-conducting power of *ſteam* and of *flame*, in explaining the effects of a blow-pipe in increaſing the action of pure flame; and in inveſtigating the moſt advantageous forms for boilers: and in the third Chapter of this Eſſay I have endeavoured to apply the diſcoveries which had been made, reſpecting the manner in which Heat is propagated in *water*, in explaining the means which appear to have been uſed by the Creator of the world for equaliſing the temperatures of the different climates, and preventing the fatal effects of the extremes of heat and of cold on the ſurface of the globe. But a moſt intereſting application remains to be made of theſe diſcoveries, to *chemiſtry;*—*vegetation;*—and the *animal economy;*—and to the learned in thoſe branches of ſcience I beg leave moſt earneſtly to recommend them. If I am not much miſtaken they will throw a new light on many of thoſe myſterious operations of Nature, in which *inanimate bodies* are put in motion—their forms changed—their component parts ſeparated, and new combinations formed; and it is poſſible that they may even enable us to account, on mechanical principles, for thoſe ſurpriſing appearances of preference and predilection among bodies, which without ever having been attempted to be explained, have been diſtinguiſhed by the appellation of *chemical affinity*.

Perhaps it will be found that every change of form, in every kind of ſubſtance, is owing to Heat; and to Heat alone:—that every concretion is a true *congelation*, effected by cold, or a diminution of Heat;—and that every change from a ſolid to a fluid form is a real *fuſion*. That the difference between calcination in the *wet* and in the *dry* way, is, in fact, much leſs than has hitherto been generally imagined; and that no metal is ever diſſolved till it has *firſt been melted*.

Perhaps it will be found, that the apparent violence with which ſolid bodies of ſome kinds are attacked by their liquid ſolvents,—and which has, I believe, been conſidered as a proof of a ſtrong chemical affinity—is not owing to any particular attraction, or election, but to the conſiderable degree of heat, or of cold, which is produced in their union with their menſtrua; or to a great difference in the ſpecific gravity of the menſtruum in its natural ſtate, and that of the ſame fluid after it has been changed to a ſaturated ſolution.

If Fluids are non-conductors of Heat, it is evident that, if any change of temperature takes place in chemical ſolution, it muſt neceſſarily produce *currents* in the ſolvent; and that theſe currents muſt be the more rapid, as the change of temperature is greater; and as they neceſſarily cauſe a ſucceſſion of freſh particles of the ſolvent to come into contact with the ſolid, it is evident,—all other things being equal,—that the rapidity of the proceſs of ſolution will be as the rapidity of theſe currents,—or as the change of temperature.

But the currents produced by the difference in the ſpecific gravities of the fluid menſtruum, and of the ſaturated ſolution, have perhaps, in general, a ſtill greater effect in bringing a rapid ſucceſſion of freſh particles of the menſtruum into contact with the ſolid body that is diſſolved in it, than thoſe produced by the change of temperature.

When theſe two cauſes conſpire to accelerate the motion of the ſame current, or when their tendencies are *in the ſame direction*, as is the caſe in the ſolution of common ſea-ſalt in water,—the ſolution ought to be moſt rapid.

When common ſalt is diſſolved in water, the ſpecific gravity of the ſaturated ſolution is greater than that of pure water, and will therefore deſcend in it; and cold being produced in the proceſs, and water being a non-conductor of Heat, the ſpecific gravity of the ſaturated ſolution will be *ſtill farther increaſed*, in conſequence of its condenſation with this cold, by which its deſcent in the water will be ſtill farther accelerated.

A curious queſtion here preſents itſelf, which, could it be reſolved, might greatly tend to elucidate this abſtruſe ſubject of philoſophical inveſtigation. Suppoſing that, in a caſe where Heat is generated in the ſolution of a ſolid in a fluid menſtruum, the *addition* to the ſpecific gravity of the menſtruum, ariſing from its chemical union with the ſolid, ſhould ſo preciſely counter-balance the *diminution* of the ſpecific gravity of the Fluid, by the Heat generated in the proceſs, that the *hot* ſaturated ſolution ſhould be preciſely of the ſame ſpecific gravity as the *cold* menſtruum;—would, or would not the proceſs of ſolution be poſſible under ſuch circumſtances?

If the *apparent* tendency to approach each other, which we ſometimes perceive in ſolids and their fluid menſtrua, were real;—if that peculiar kind of attraction of predilection which has been called chemical affinity, has a real exiſtence, and if its influence reaches *beyond the point of actual contact*, (as has, I believe, been generally ſuppoſed,) as there is no appearance of any attraction whatever, or affinity, between any ſolid body, and a ſaturated ſolution of the ſame body in its proper menſtruum, it ſeems probable that the ſolution would take place, —under the circumſtances deſcribed: but ſhould the attraction of affinity, according to the definition of it here given, have no exiſtence in fact, (which is what I very much ſuſpect,) in that caſe it is evident that the ſolution, though it would not be abſolutely impoſſible, would be ſo very ſlow as hardly to be perceptible.

It would not be *impoſſible*, becauſe the particles of the menſtruum in immediate contact with the ſolid, though, in the moment of their ſaturation, they would have no tendency to move out of their places, yet, as they would by degrees neceſſarily give off, to the undiſſolved part of the ſolid, a part of the Heat acquired in the chemical proceſs by which they were ſaturated, being condenſed by this loſs of Heat, they would, at length, begin to deſcend, and give place to other particles of the menſtruum; which, in their turns, would follow them, but with velocities, however, continually decreaſing;—on account of the gradual augmentation of temperature of the undiſſolved part of the ſolid, and of the Heat communicated by that ſolid ſubſtance to the whole maſs of the liquid menſtruum.

Though it would, probably, be extremely difficult to contrive any ſingle experiment, from the reſult of which a ſatisfactory deciſion of this queſtion could be obtained, yet it does not appear to be impoſſible to diſcover by *indirect means*, the principal fact on which its deciſion muſt depend.

It is a well known fact, that, when water which holds ſea-ſalt in ſolution is mixed, in any veſſel, with freſh water, the ſalt will, after a ſhort time, be found to be very equally diſtributed in every part of the whole maſs; and I believe that it has been generally conſidered, that this equal diſtribution of the ſalt is owing to the *affinity* which is ſuppoſed to exiſt between ſea-ſalt and water.

Having doubts with reſpect to the exiſtence of this ſuppoſed attraction; and ſuſpecting that the equal diſtribution of the ſalt was owing to a very different cauſe—the internal motions among the particles of the water, occaſioned by accidental changes of temperature—I made the following Experiment, which, I fancy, will be conſidered as deciſive.

Experiment, No. 57.

I took a cylindrical glaſs jar, 4½ inches in diameter, and 7¼ inches high, and placing it in the middle of another cylindrical glaſs jar, 7½ inches in diameter, and 8 inches high, which ſtood in a very ſhallow earthen diſh, nearly filled with pounded ice and water, I placed the diſh, with its contents, on a ſtrong table, in an uninhabited room, in a retired part of the houſe, where the temperature of the air, which was the ſame, with very little variation, day and night, was at about 36° F. Having prepared, and at hand, a quantity of the ſtrongeſt *brine* I could make with ſea-ſalt, which was very clear, tranſparent, perfectly colourleſs, and ice-cold; and alſo, a quantity of freſh, or pure water,—ice-cold, lightly tinged of a red colour with turnſol; and ſome ice-cold *olive-oil;* I firſt poured as much of the freſh water into the ſmall cylindrical jar as was neceſſary to fill it up to the height of above 2 inches; and then, by means of a glaſs funnel, which ended in a long and narrow tube, by introducing this tube into the freſh water, and reſting it on the bottom of the jar, I poured a quantity of the brine, equal to that of the freſh water, into the jar; and in performing this operation I took ſo much care to do it gently, and without diſturbing the freſh water already in the jar, that, when it was finiſhed, the freſh water, which, as it was coloured red, could eaſily be diſtinguiſhed from the brine, remained perfectly ſeparated from this heavier ſaline liquor; on which it repoſed quietly, without the ſmalleſt appearance of any tendency to mix with it.

I now filled, to the height of about 5 inches, the void ſpace between the outſide of the ſmall jar and the inſide of the large jar in which it was placed, with ice-cold water, mixed with a quantity of ice, in pieces as large as walnuts,—(pounded ice would have obſtructed the view in obſerving, through the ſides of the large jar, what paſſed in the ſmaller)—and when this was done, I very carefully poured ice-cold *olive oil** into the ſmaller jar till it covered the ſurface of the (tinged) freſh water to the height of about an inch (ſee Fig. 5. Plate IV.); and placing myſelf near the table, in a ſituation where I had a diſtinct view of the contents of the ſmall jar, I ſet myſelf to obſerve the reſult of the Experiment.

After waiting above an hour without being able to perceive the ſmalleſt appearance of any motion, either in the brine, or in the freſh water, (the one continuing to repoſe on the other with the moſt perfect tranquillity, and without the ſmalleſt diſpoſition to mix together) I left the room.

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* This oil ſerved not only to keep the water on which it repoſed, quiet; but alſo to prevent any communication of heat between it and the air of the atmoſphere.

When I returned to it the next day, I found things preciſely in the ſtate in which I had left them; and they continued in this ſtate, without the ſmalleſt appearance of any change, or of any diſpoſition to change, during *four days*.

At the end of that time, thinking that any farther prolongation of the Experiment would be quite uſeleſs, I removed the ſmall jar, taking care not to agitate its contents, and placed it in the window of a room heated by a German ſtove.

In leſs than an hour I perceived that the brine and the (tinged) freſh water began to mix, and at the end of 24 hours they were intimately mixed throughout, as was evident by the colour of the aqueous fluid on which the oil repoſed; which now appeared to the eye to form one uniform maſs of a light red tint.

I ſhall leave it to philoſophers to draw their own concluſions from the reſults of this Experiment: In the mean time, there is one fact which it ſeems to point out that I ſhall juſt mention, which is not only curious in itſelf, but may lead to very important diſcoveries. It appears to me to afford ſtrong reaſons to conclude that, were a lake but *very deep*, its waters, near the ſurface, would neceſſarily be freſh, even though its bottom ſhould be one ſolid maſs of rock ſalt.

Would it be ridiculous to make Experiments to determine whether the water at the bottom of ſome very deep lakes is not impregnated with ſalt?

Should it be found to be actually the caſe, it might prove an unexhauſtible treaſure in an inland country, where ſalt is ſcarce.

As mines of rock-ſalt are often found in the neighbourhood of freſh lakes, it ſeems reaſonable to ſuppoſe that the waters of ſuch lakes ſhould *ſometimes* be in contact with *ſtrata* of theſe mines; and when I firſt began to meditate on this ſubject, I was much ſurpriſed,—not that the ſalt water which may lie at the bottom of freſh lakes ſhould not already have been diſcovered,—for from the firſt I plainly perceived that nothing could happen in the ordinary courſe of things that could bring it to the light, or even afford any grounds to ſuſpect its exiſtence;—but, as *ſtrata* of ſalt mines frequently lie higher than the mean level of the country, I was ſurpriſed that lakes *of ſalt water* ſhould not more frequently be found; and as theſe reflections occurred to me *after* I had diſcovered what appeared to me to be an evident proof of the wiſdom and goodneſs of the Creator in *making* all lakes in cold countries *freſh*, I began to be alarmed for the fatal conſequences that might enſue, if, by chance, the ſide of a lake ſhould come into contact with a mountain of ſalt; as I ſaw might eaſily happen.

Shall I,—or ſhall I not attempt to give my reader an idea of what I felt, when, meditating on the ſubject, and almoſt beginning to repent of what many, no doubt, have already condemned as the fooliſh dream of an enthuſiaſtic imagination, I ſaw, all at once, that the moſt effectual care had been taken to prevent the evils I apprehended;—that from the very conſtitution of things, and the ordinary and uniform operation of the known laws of Nature, the permanent *exiſtence of a lake*, SALT AT THE SURFACE, is *abſolutely impoſſible ;* even though it ſhould be ſurrounded on every ſide by mountains of ſalt * ?

Though the exploſion of a volcano, an earthquake, or any other great convulſion, by which the ſhores of a lake might be brought into contact with a vaſt mine of ſalt, might cauſe the whole maſs of its water to be ſalt for a time; yet, the evil would ſoon effect its own remedy: The falling in of the cruſt of earth and ſtones by which mines of ſalt are every where found to be covered, (and without which they could not exiſt) would very ſoon cover the naked ſalt, and the water *at the ſurface of the lake* would again become perfectly freſh. Should, however, the lake be ſo deep that the temperature at the bottom ſhould remain the ſame ſummer and winter, without any ſenſible variation, it is moſt certain that its waters *there*—(at the bottom of the lake)—would remain perfectly ſaturated with ſalt for ever.

But are there not ſome reaſons to conclude that the water at the bottoms of *all very deep lakes* ought neceſſarily to be ſalt, even in ſituations where there are no mines of ſalt near?

The ſea-ſhells that are frequently found in high inland ſituations, as well as many other appearances noticed by naturaliſts, ſtrongly indicate that moſt of our continents have been covered by the waters of the ocean. Now if that event ever happened—however remote the period may be at which it took place—it ſeems highly probable that the ſalt water left at the bottoms of all deep lakes, by the ſea, on its retiring, *muſt be there now.*

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* By the word *Lake* I mean, as is eaſy to perceive, a collection of water, in a high inland ſituation, from which there is a conſtant efflux.

I cannot take my leave of this ſubject without juſt obſerving, that the diſcovery of the *impoſſibility* of the permanent exiſtence of what we can plainly perceive would be an evil, certainly ought not to *diminiſh* our admiration of the wiſdom of the great Architect of the Univerſe.

CHAP. II.

*Water made to congeal at its under Surface.—Obſervation reſpecting the Formation of Ice at the Bottoms of Rivers.—Reaſons for concluding that Heat can never be equally diſtributed in any Fluid.—Perpetual Motions occaſioned in Fluids by the unequal Diſtribution of Heat.—An inconceivably rapid Succeſſion of Colliſions among the integrant Particles of Fluids is occaſioned by the internal Motions into which Fluids are thrown in the Propagation of Heat.—An Attempt to eſtimate the Number of thoſe Colliſions which take place in a given Time.—Theſe Inveſtigations will greatly change our Ideas reſpecting the real State of Fluids apparently at reſt.—*FLUIDITY *may be called the* LIFE OF INANIMATE BODIES.—*Conjectures reſpecting the* VITAL PRINCIPLE *in Living Animals; and the Nature of Phyſical* STIMULATION.

WHATEVER the mechanical operation may in fact be, by which thoſe effects are produced that have given riſe to the idea of the exiſtence of an attraction of affinity—(a power different from gravitation)—between ſolid bodies and their liquid menſtrua, and between different portions of the ſame menſtruum differently ſaturated; the reſult of the foregoing Experiment (No. 57) proves that two particles of water in combination with very different quantities of ſea-ſalt; or a particle of water *ſaturated* with ſalt, and another perfectly free from ſalt, *may be* in contact with each other for any length of time without ſhowing any appearance of a diſpoſition to equalize the ſalt between them.

But ſhould we even admit as a fact, what this Experiment ſeems to indicate, namely, that there is no ſuch thing as an *attraction of predilection* between ſolids and their ſolvents; and that all thoſe motions which have been attributed to the action of that ſuppoſed power,—(as well as all other motions which take place in Fluids,)—are the immediate effects of *gravitation* acting according to immutable laws, and *changes of ſpecific gravity by Heat;* yet there would ſtill remain one great difficulty in explaining chemical ſolution. As all mechanical operations require *a certain time* for their performance; and as the motion which is occaſioned in a Fluid by a change of ſpecific gravity in any individual particles of it, *begins* as ſoon as the change begins to take place, if there be no attraction between the particles of ſolid bodies and the particles of their menſtrua;—as Heat is ſuppoſed to be generated or abſorbed, or, to ſpeak more properly,—both generated and abſorbed,—in the *contact* of thoſe particles, and previous to the completion of their chemical union; how does it happen that the particle of the menſtruum whoſe ſpecific gravity is neceſſarily changed by this change of temperature, does not *immediately* quit the ſolid, in conſequence of this change; and before the proceſs of ſolution has *had time to be completed?*

A conſideration of the effects of the *vis inertiæ* of the particle of the menſtruum whoſe ſpecific gravity is thus changed, and alſo of the *vis inertiæ* of the reſt of the Fluid, and the reſiſtance it muſt oppoſe to the motion of its individual ſolitary particles, would furniſh us with arguments that might be employed with advantage in removing this difficulty; but I fancy that the reſult of the Experiment of which I ſhall preſently give an account will be more ſatisfactory than any reaſoning, unſupported by facts, that I could offer on the ſubject.

When a doubt ariſes with regard to the *poſſibility* of any operation of a peculiar kind, which is *ſuppoſed* to take place, in any proceſs of nature among thoſe infinitely ſmall integrant particles of bodies, which eſcape, and muſt ever eſcape, the cognizance of our groſs organs, however they may be aſſiſted by art, the ſhorteſt way of deciding the queſtion is to put the known powers of nature in action under ſuch circumſtances that the effects produced by them muſt ſhow, unequivocally, whether the ſuppoſed operation be poſſible, or not: and if it be found to be poſſible in one caſe, we may then argue with leſs diffidence on the probability of its actually taking place in the ſpecific caſe in queſtion.

It has been abundantly proved by the Experiments of M. de Luc, and by thoſe of my friend Sir Charles Blagden, that when water, in cooling, has arrived at the temperature of about 41° F. its condenſation with cold ceaſes, and it begins to expand; and continues to expand gradually as its temperature goes on to be farther diminiſhed, till it is changed to ice. Availing myſelf of that moſt important diſcovery, I made the following Experiment.

Experiment, No. 58.

Having poured *mercury*, at the temperature of 60°, into a common glaſs-tumbler, till this Fluid ſtood at the height of about an inch; I then poured about twice as much water (at the ſame temperature) upon it; and placing the tumbler in a ſhallow earthen diſh, ſurrounded it to the height of the level of the ſurface of the mercury with a freezing mixture of ſnow and common ſalt. Having done this, I was very curious indeed to ſee in what part of the water ice would firſt make its appearance.

Could it be at the upper ſurface of it? That appeared to me to be impoſſible; for the Experiment being made in a room warmed by a German ſtove, the temperature of the air which repoſed on that ſurface was conſiderably above the point at which water freezes.

Could it be at its lower ſurface, where it reſted on the upper ſurface of the mercury?—If that ſhould happen, it would ſhow, that, notwithſtanding the diminution of the ſpecific gravity of the water in paſſing from the temperature of 41° to that of 32°; and the tendency which this diminution gave it to quit the ſurface of the mercury from the inſtant when, it being cooled by a contact with it, it had paſſed the point of 41°; yet there was time ſufficient for the congelation to be completed *before the particle of water ſo cooled could make its eſcape.*

The reader will naturally conclude from what was ſaid in the preceding page, that it was merely with a view to the determination of that ſingle fact, that this Experiment was contrived; and he will perceive by the reſult of it that my expectations with regard to it were fully anſwered.

Ice was not only formed *at the bottom of the water*, at its under ſurface, where it was in contact with the cold mercury; but, I found on repeating the experiment, and varying it, by previouſly cooling the mercury in the tumbler to about 10°, that *boiling-hot water*, poured gently upon it, was inſtantly frozen, and gradually formed a thick cake of ice, covering the mercury; though almoſt the whole of the maſs of the unfrozen water, which reſted on this ice, remained nearly boiling-hot.

This Experiment not only determines the point for the deciſion of which it was undertaken; but alſo enables us to form a juſt opinion reſpecting a matter of fact which has been the ſubject of a good deal of diſpute.

Though many accounts have been publiſhed of ice found at the bottom of rivers, yet doubts have been entertained of the poſſibility of its being *formed* in that ſituation. From the reſult of the foregoing Experiment it appears to me that we may ſafely conclude, that, if after a very long and a very ſevere froſt, by which the ſurface of the ground has not only been frozen to a conſiderable depth, but alſo cooled ſeveral degrees below the freezing point, a river ſhould overflow its banks, and cover the ſurface of ground *previouſly ſo cooled*, ice would be formed at the bottom of the water: but all the Experimeuts that have been made on the congelation of water ſhow the abſolute impoſſibility of ice being ever formed, in any country, at the bottom of a river *which conſtantly fills its banks*, or which never leaves its bed expoſed, dry, to the cold air of the atmoſphere.

By reflecting on the various conſequences that ought to follow from the peculiar manner in which Heat appears to be propagated in Fluids, we are led to conclude, that it is almoſt impoſſible that any Fluid expoſed to the action of light ſhould ever be throughout of the ſame temperature, though its maſs be ever ſo ſmall; and that the difference in the Heat of its different particles muſt occaſion perpetual motions among them.

Suppoſe any open veſſel,—as a common glaſs tumbler for inſtance,—containing a piece of money, a ſmall pebble, or any other ſmall ſolid opaque body, to be filled with water, and expoſed in a window, or elſewhere, to the action of the ſun's rays. As a ray of light cannot fail to generate Heat *when* and *where* it is ſtopped or abſorbed, the rays, which, entering the water, and paſſing through it, impigne againſt the ſmall ſolid opaque body at the bottom of the veſſel, and are *there abſorbed*, muſt neceſſarily generate a certain quantity of Heat; a part of which will penetrate into the interior parts of the ſolid, and a part of it will be communicated to thoſe colder particles of the water which repoſe on its ſurface.

Let us ſuppoſe the quantity of Heat ſo communicated to one of the integrant particles of the water to be ſo ſmall, that its effect in diminiſhing the ſpecific gravity of the particle is but juſt ſufficient to cauſe it to move upwards in the maſs of the liquid with the very ſmalleſt degree of velocity that would be perceptible by our organs of ſight, were the particle in motion large enough to be viſible. This would be at the rate of about *one hundredth part of an inch* in a ſecond.

This velocity, though it appears to us to be ſlow in the extreme, when we compare it with *thoſe* motions that we perceive among the various bodies by which we are ſurrounded, yet we ſhall be ſurpriſed when we find what a rapid ſucceſſion of events it is capable of producing.

If we ſuppoſe the diameter of the integrant particles, or *molécules* of water, to be *one millionth part of an inch*—(and it is highly probable that they are even leſs—*)—in that caſe, it is moſt certain that an individual particle, moving on in a quieſcent maſs of that Fluid with the velocity in queſtion, namely, at the rate of $\frac{1}{100}$ part of an inch in 1 ſecond, would run through a ſpace equal to

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* Leaf gold, ſuch as is prepared and ſold by the gold-beaters, is not *four times* as thick as the diameter here aſſumed for the integrant particles of water. Theſe leaves of ſolid metal have been found by computation to be no more than $\frac{1}{282000}$ of an inch in thickneſs. How much leſs muſt be the diameter of the integrant particles of gold?

ten thouſand times the length of its diameter in *one ſecond*, and conſequently would come into contact with at leaſt *ſix hundred thouſand* different particles of water in that time.

Hence it appears how inconceivably ſhort the time muſt be that an individual particle, in motion, of any Fluid, can remain in contact with any other individual particle, not in motion, againſt which it ſtrikes in its progreſs (however ſlow that progreſs may appear to us to be) through the quieſcent maſs of the Fluid!

Suppoſing the contact to laſt as long as the moving particle employs in paſſing through a ſpace equal to the length of its diameter—which is evidently all that is poſſible; and more than is probable;—then, in the caſe juſt ſtated, the contact could not poſſibly laſt longer than $\frac{1}{10000}$ part of a ſecond! This is the time which a cannon bullet, flying with its greateſt velocity, (that of 1600 feet in a ſecond,) would employ in advancing 2 inches.

If the cannon bullet be a *nine pounder*, its diameter will be four inches; and if it move with a velocity of 1600 feet (=19200 inches) in a ſecond, it will paſs through a ſpace juſt equal to 4800 times the length of its diameter in 1 ſecond. But we have ſeen that a particle of water moving $\frac{1}{100}$ of an inch in a ſecond actually paſſes through a ſpace equal to 10000 times the length of its diameter in that time: Hence it appears that *the velocity with which the moving body quits the ſpaces it occupies* is more than twice as great in the particle of water, as in the cannon bullet!

There is one more computation which may be of uſe in enabling us to form more juſt ideas of the ſubject under conſideration,—and ſurely too much cannot be done to enlighten the mind, and aſſiſt the imagination, in our attempts to contemplate thoſe inviſible operations of nature which nothing but the ſharpeſt ken of the intellectual eye will ever be able to detect and ſeize.

As ſucceeding events which fall under the cognizance of our ſenſes cannot be diſtinguiſhed if they happen oftener than about *ten times in a ſecond* *, it appears that when a particle of water moves in a quieſcent maſs of that fluid at the rate of $\frac{1}{100}$ part of an inch only, in one ſecond, its ſucceeding colliſions with the different particles, at reſt, of that fluid, againſt which it ſtrikes as it moves on, muſt be ſo inconceivably rapid that no leſs than *one thouſand* of them muſt actually take place, *one after the other*, in the ſhorteſt ſpace of time that is perceptible by the human mind †.

After

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* This aſſertion, in as far, at leaſt, as it relates to objects of ſight, may be proved by the following eaſy experiment: Let a wheel, with any known number of ſpokes, be turned round its axis with ſuch a velocity as ſhall be found neceſſary, in order that the ſpokes may diſappear or become inviſible.—From the velocity of the wheel, and the number of ſpokes in it, the fact will be decided.

† It probably will not eſcape the obſervation of my learned readers, that the velocity which I have here aſſigned to the ſingle particle of water, moving upwards in that fluid in conſequence of a change of its ſpecific gravity by Heat, though apparently very ſmall,—($\frac{1}{100}$ part of an inch in a ſecond,)—is, however, moſt probably conſiderably greater, in fact, than any individual *ſolitary* particle of that fluid could poſſibly acquire, in the ſuppoſed circumſtances, by any change of temperature, however great, owing to the reſiſtance which

would

After we have patiently examined the reſult of theſe inveſtigations, and the imagination has become *familiarized* with the contemplation of the intereſting facts they preſent to it, how much will our ideas be changed with regard to the real ſtate of fluids apparently at reſt! They will then appear to us to be, what no doubt they really are in fact, an aſſemblage of an infinite number of infinitely ſmall particles of matter moving continually, or without ceaſing, and with inconceivable velocities.

We ſhall then conſider fluidity as the *life of inanimate bodies*, and congelation as the *ſleep of death;* —and we ſhall ceaſe to aſcribe active powers, or exertions of any kind, to dead motionleſs matter.

But what ſhall we think of the *vital principle* in living animals?—Does not their life alſo depend on the internal motions in *their* fluids, occaſioned by an *unequal* diſtribution of heat?—And is not

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would neceſſarily be oppoſed to its motion by the quieſcent particles of the fluid. Aware of this objection, and being deſirous of being prepared to meet it, I took ſome pains to compute, by the rules laid down by Sir Isaac Newton in his *Principia*, book ii. ſect. vii., what the greateſt velocity is that a ſolitary particle of water (ſuppoſed to be $\frac{1}{1000000}$ of an inch in diameter) could poſſibly acquire by a given change of its ſpecific gravity: And I found that if the ſpecific gravity of water at the temperature of 32° F. be taken at 1.00082, and its ſpecific gravity at 80°, at 0.99759, as lately determined by accurate experiments, then, a ſingle particle of water at the temperature of 80°, ſituated in a quieſcent maſs of that fluid at 32°, the greateſt velocity this hot particle could acquire in moving upwards in conſequence of its comparative levity would be that of $\frac{1}{2631}$ part of an inch in 1 ſecond. This is at the rate of about one inch and an half in 1 hour.—But it is evident, that when great numbers of particles unite and form currents, they will make their way through the quieſcent fluids with greater facility, and conſequently will move faſter.

ſtimulation, in all caſes, the mere mechanical effect of the communication of Heat?

It is an opinion which we know to be as old as the days of MOSES, that *the life of an animal reſides in its blood;* and it is highly probable that it dates from a period ſtill more remote. It was lately revived by an anatomiſt and phyſiologiſt, (now no more *,) who was eminently diſtinguiſhed for ſagacity; and it appears to me that the late diſcoveries reſpecting the manner in which Heat is propagated in Fluids tend greatly to elucidate the ſubject; and to give to the hypotheſis a high degree of probability.

According to this hypotheſis—(as it may now be explained)—every thing that increaſes the *inequality of the diſtribution* of the Heat in the maſs of the blood—(even though it ſhould not immediately augment its quantity)—ought to increaſe the intenſity of thoſe *actions* in which life conſiſts. But are there not many ſtriking proofs that this is the caſe in fact?

Do not *reſpiration,*—*digeſtion,*—and *inſenſible perſpiration* all tend evidently—(that is to ſay, according to our aſſumed principles, with regard to the manner in which Heat is propagated in Fluids)—to *produce*, and to *perpetuate* this inequality of heat in the animal fluids? And do we not ſee what an immediate and powerful effect they have in increaſing the intenſity of the action of the powers of life?

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* Mr. John Hunter.

If animal life depends eſſentially on thoſe *internal* motions in the animal fluids,—which, as has been ſhown,—are occaſioned by the difference of the *ſpecific gravities* of their integrant particles, or *molécules*, ariſing from their different temperatures;—in that caſe, it is evident that the *vital powers* would be ſtrengthened, or their action increaſed, either by *heat*, or by *cold*, properly applied. But is not this found to be the caſe in fact? Does not the *dram of brandy* at St. Peterſburgh produce the ſame effects as the *draught of iced lemonade* at Naples, and by the ſame mechanical operation, but acting in oppoſite directions? And does not the *loſs of Heat*, by inſenſible perſpiration, contribute as efficaciouſly to the preſervation of that *inequality of temperature* which is eſſential to life, as the *introduction of Heat* into the ſyſtem in reſpiration?

Is not the ſudden coagulation of blood, when drawn from a living animal, and are not all the other rapid changes that take place in it, evident proofs of an unequal diſtribution of Heat? And does not the *viſcoſity* of blood, as well as its perpetual motions in the vaſcular ſyſtem, contribute very powerfully to the preſervation of that inequality?

Are not the livid ſpots on the ſurface of the body, which indicate a beginning of mortification, produced in conſequence of a ſeparation, or *precipitation* of the heterogeneous particles of the animal Fluids, according to their ſpecific gravities and individual temperatures, occaſioned by reſt, or an interruption of circulation? And may we not emphatically pronounce ſuch Fluids to be *dead?*

Would not any liquid in which Heat were *equally diſtributed* be a *fatal poiſon* if injected into the veins of a living animal? And would not this be the caſe even were the liquid ſo injected a portion of the animal's own blood, or of the lymph or any other of its component parts, and were it at the mean temperature preciſely of the healthy Fluids circulating in the veins and arteries of the animal?

Is not glandular ſecretion a true precipitation? and is it not poſſible that the formation of the ſolids, and the growth of an animal body, may be effected by a proceſs exactly ſimilar to congelation? And are there not even circumſtances from which we might conclude, with a conſiderable degree of probability, that moſt of theſe congelations are formed at or about the temperature of boiling water?

But I forbear to enlarge on this ſubject. I find I have unawares entered a province, where, if I advance farther, I ſhall certainly be expoſed to the danger of being conſidered and treated as an intruder; and I muſt haſten to make my retreat, which I ſhall endeavour to effect by abruptly putting an end to this Chapter.

CHAP. III.

Probability that intenſe Heat frequently exiſts in the ſolitary Particles of Fluids, which neither the Feeling nor the Thermometer can detect.—The Evaporation of Ice during the ſevereſt Froſt explained on that Suppoſition.—Probability that the Metals would evaporate when expoſed to the Action of the Sun's Rays were they not good Conductors of Heat.—Mercury is actually found to evaporate under the mean temperature of the Atmoſphere.—This Fact is a ſtriking Proof that FLUID MERCURY *is a Non-conductor of Heat.—Probability that the Heat generated by the Rays of Light is always the ſame in Intenſity; and that thoſe Effects which have been attributed to Light ought perhaps in all Caſes to be aſcribed to the Action of the Heat generated by them—A ſtriking Proof that the moſt intenſe Heat does ſometimes exiſt where we ſhould not expect to find it.—Gold actually melted by the Heat which exiſts in the Air of the Atmoſphere, where there is no Appearance of Fire, or of any Thing red-hot.—We ought to be cautious in attributing to the Action of unknown Powers, Effects ſimilar to thoſe produced by the Agency of Heat.—The moſt intenſe Heat may exiſt without leaving any viſible Traces of its Exiſtence behind it.—This important Fact illuſtrated by the neceſſary Reſult of an imaginary Experiment.*

How far the poſſibility of the communication of Heat between the integrant particles of a Fluid may or may not be owing to the extreme mobility of thoſe particles, and to the infinitely ſhort time that two of them, of different ſpecific gravities, (owing to a difference of temperature) can remain in contact, I leave others to determine; in the mean time, it is moſt certain that the exiſtence of this impoſſibility of any immediate communication of Heat among the particles of a Fluid renders the diſtribution of Heat very unequal; and it ſeems highly probable that many appearances which have been attributed to very different cauſes, are in fact owing to *intenſe Heat* exiſting and producing the effects proper to it in ſituations where its exiſtence has not even been ſuſpected.

If Fluids are non-conductors of Heat, no ſituation can poſſibly be more favourable to its preſervation than when it exiſts in them; and it is not only evident *a priori* that the moſt intenſe Heat *may exiſt* in a few ſolitary particles of ſome Fluids, without its being poſſible for us to detect it, or to diſcover the fact, either by our feeling or by the thermometer; but there are many appearances that ſtrongly indicate,—and others that prove, that intenſe Heat actually does exiſt *in that* concealed or imperceptible ſtate very often.

There is no reaſon to ſuppoſe that it is poſſible for ice to be reduced to ſteam without being previouſly melted; and it is well known that ice cannot be melted with a lower degree of Heat than that of 32° of Fahrenheit's ſcale: but in the midſt of winter, in the coldeſt climates, and when the temperature of air of the atmoſphere, as ſhown by the thermometer, has been much below 32°, ice, expoſed to the air, has been found to evaporate.

How can we account for this event, except it be by ſuppoſing that ſome of the particles of air, which accidentally (as we expreſs it) come into contact with the ice, are ſo hot, as not only to melt the ſmall particles of ice which they happen to touch, but alſo to reduce a part of the generated water to ſteam, before it has time to freeze again; or by ſuppoſing that this is effected by intenſe Heat generated by light abſorbed by ſmall projecting points of the ice? As ice is a very bad conductor of Heat, that circumſtance renders it more likely that the event in queſtion ſhould actually take place, in either of theſe ways.

If the metals were very bad conductors of Heat, inſtead of being very good conductors of it, I think it more than probable that even *they* would be found to evaporate, when expoſed to the action of the direct rays of the ſun; and perhaps alſo in ſituations in which ſuch an event would appear ſtill more extraordinary.

Mercury has been actually found to *evaporate* under the mean temperature of the atmoſphere!—What a ſtriking proof is this that *fluid mercury* is a non-conductor of Heat;—and alſo, that very intenſe Heat may be generated, or exiſt, where it would not naturally be expected to be found. And does not the evaporation of water under the mean temperature of the atmoſphere afford another proof of this laſt fact?

That the moſt intenſe Heat is often excited in very ſmall particles of ſolid bodies diſperſed about in the midſt of maſſes of cold liquids is not to be doubted. It is well known what an intenſe Heat the rays of the ſun are capable of exciting; and it ſeems to be highly probable that Heat actually excited by them is always the ſame—that is to ſay—*intenſe in the extreme*: but when the rays are few, and when circumſtances are not favourable to the *accumulation* of the Heat they generate, it is often ſo ſoon diſperſed, that it eſcapes the cognizance of our ſenſes, and of our inſtruments; and ſometimes leaves no viſible traces of its exiſtence behind it.

Why ſhould we not ſuppoſe that the Heat generated by a ray of light, which, entering a maſs of cold water, accidentally meets with an infinitely ſmall particle of any ſolid or opaque ſubſtance which happens to be floating in the liquid, and is abſorbed by it, is not juſt as intenſe as that generated in the focus of the moſt powerful burning mirror, or lens?

Mr. Senebier has given us an account of a great number of intereſting Experiments on the effects produced on different bodies by expoſure to the direct rays of the ſun; but why may we not attribute all thoſe effects to the intenſe *local* Heat, generated by the light abſorbed by the infinitely ſmall—and, if I may uſe the expreſſion—*inſulated* particles of the bodies which were found to be affected by it?

The ſurface of wood of various kinds was turned brown. The ſame appearances might be produced in a ſhorter time by the rays which proceed from a red-hot iron, which change the ſurface of the wood to an imperfect coal. But were not the ſurfaces of the woods which were turned brown by the light of the ſun in Mr. Senebier's experiments changed to an imperfect coal?—And is it poſſible for a Heat leſs intenſe than that of *incandeſcence* to produce that effect?

Among the many facts that might be adduced to prove that the moſt intenſe Heat *may*, and frequently *does exiſt* where we ſhould not expect to find it, the following appears to me to be very ſtriking and convincing. It is, I believe, generally imagined that the intenſity of the Heat generated in the combuſtion of fuel is much leſs in a ſmall fire, than in a great one; but there is reaſon to think that this is an erroneous opinion, founded on appearances that are not concluſive; at leaſt it is certain that the intenſe Heat of a large ſmelting furnace, ſuch as is neceſſary for melting the moſt refractory metals, actually exiſts in the feeble flame of the ſmalleſt candle:—and what may appear ſtill more extraordinary,—this intenſe degree of Heat often exiſts in the air of the atmoſphere, *where no viſible ſigns of Heat appear*, as I ſhall preſently ſhow.

Iron is fully *red-hot* by day-light at the temperature of about 1000° of Fahrenheit's ſcale; braſs melts at 3807°,—copper at 4587°,—ſilver at 4717°,—and gold at 5237°; and nothing is more certain than that the Heat muſt be at that intenſity which correſponds to the 5237th degree of Fahrenheit's ſcale, *where gold is found to melt.* But very fine gold, ſilver, or copper wire, flatted, (ſuch as is uſed to cover thread to make lace,) melts inſtantaneouſly on being held in the flame of a candle. It will even be melted if it be held a few ſeconds *over* the flame of a candle, *at the diſtance of more than an inch from the top of the flame,* in a place where there is no appearance of fire, or of any thing red-hot.

From the important information which we acquire from the reſult of theſe ſimple Experiments, we ſee how much we ought to be on our guard in forming an opinion with reſpect to the *intenſity* of the Heat which *may exiſt* in the inviſible inſulated particles of matter of any kind that may be ſcattered about in a given ſpace,—or which may float in any Fluid, where neither our feeling nor our thermometers can poſſibly be ſenſibly affected by it,

A thermometer can do no more than indicate the *mean of the different temperatures of all thoſe bodies or particles of matter which happen to come into contact with it.* If it be ſuſpended in air, it will indicate the mean of the temperatures of thoſe particles of air *which happen to touch it;* but it can never give us any information reſpecting the *relative* temperatures of thoſe particles of air.

If, during the moſt intenſe froſt, a thermometer were ſuſpended in the neighbourhood of a burning candle,—

candle,—in the ſame room for inſtance,—if it were placed over the candle, or nearly ſo, though it ſhould be diſtant from it ſeveral feet, as air is a non-conductor of Heat, there is not the ſmalleſt doubt but that ſome ſolitary particles of air heated by the candle to the intenſe Heat of melting gold, would reach the thermometer; but neither the thermometer, nor the hand held in the ſame place, could give us any indication of ſuch an event.

As it appears from all that has been ſaid that intenſe Heat *may exiſt* even under the form of *ſenſible Heat*, where its preſence cannot be diſcovered or detected by us; and as it ſeems highly probable that in many caſes, where its exiſtence may eſcape our obſervation, it may nevertheleſs be capable of producing very viſible effects, I think we ought always to be much on our guard in accounting for effects ſimilar to thoſe which are known to be produced by Heat; and never, without very ſufficient reaſons, attribute them to the agency of any other, *unknown* power: and this caution appears to me to be peculiarly neceſſary in accounting for thoſe effects which have been found to be produced in various bodies when they are expoſed to the action of the ſun's rays.

If the ſolar rays concentrated in the focus of a lens, when they are made to fall on a piece of wood, inſtantly change its ſurface to a black colour, and reduce it to charcoal, why may we not conclude that the change of colour which is gradually or more ſlowly produced in the ſame kind of wood, when it is ſimply expoſed in the ſun-beams, is produced in the ſame manner?

The difference in the *times* neceſſary to produce ſimilar effects in theſe two caſes is no proof that they are not produced *in the ſame manner;* for if they are effected merely by the agency of Heat, (which I ſuppoſe) then the effects produced in any given time will not be as the denſity of the light, or as the number of rays, but as that part of the Heat generated, which, not being immediately diſperſed or carried off by the air, has time to produce the action proper to it in the wood; and conſequently muſt be incomparably greater, in proportion, when the rays are concentrated, than when they are not.

Luna cornea expoſed to the action of light changes colour;—but why ſhould we not attribute this change to the expulſion of the oxygen united with the metal, by the agency of the Heat generated by the light? To remove every poſſible objection to this explanation of the phenomenon nothing more appears to be neceſſary than to admit, what is well known, that this metallic oxyd may be reduced, without addition, *with ſome degree of Heat,*—and that this ſubſtance is a bad conductor of Heat.

Will not the admiſſion of our hypotheſis reſpecting the *intenſity* of the Heat which is ſuppoſed to be generated where light is ſtopped, and of that reſpecting the non-conducting power of Fluids with regard to Heat, enable us to account, in a manner more ſatisfactory than has hitherto been done, for the effects of the ſun's light in bleaching linen, when it is expoſed wet to the action of his direct rays? as alſo for the reduction of thoſe metallic oxyds which have been found to be revived by expoſure to light?—And will it not alſo aſſiſt us in accounting for the production of pure air in the beautiful Experiment of Doctor Ingenhouz, in which the green leaves of living vegetables are expoſed, immerſed in water, to the ſun's rays?

Mr. Senebier has ſhown that the colouring matter of healthy green leaves of vegetables, which is extracted from them by ſpirits of wine, and which tinges the ſpirits of a beautiful green colour, is deſtroyed, or rather changed to a dirty brown colour, in a few minutes, on expoſing this tincture in a tranſparent phial, and *in contact with pure air*, to the direct rays of a bright ſun;—but why ſhould we not conſider this proceſs as a real combuſtion?

The Heat acquired by the liquid,—which, as I have often perceived in repeating the Experiment, is very conſiderable,—and the neceſſity there is for the preſence of *pure air*, that the Experiment may ſucceed, ſeem to indicate that ſomething very like combuſtion muſt take place in it.

If liquids are non-conductors of Heat, they ought certainly, *on that account*, to be peculiarly well calculated for confining, and conſequently furthering the operations of that Heat which is generated by light, or by any other means, in their integrant particles, or in the infinitely ſmall and inſulated particles of other bodies that are diſperſed about, or held in ſolution in them; as I have already more than once had occaſion to obſerve.

If this ſuppoſition be admitted, a very great difficulty will be removed in accounting for chemical ſolution on the hypotheſis that the change of form from a ſolid to a fluid ſtate is in all caſes a real fuſion; or that it is affected by the *ſole* agency of Heat; and that concretion, or cryſtallization, is a proceſs in all reſpects perfectly analogous to freezing.

There are but three forms under which ſenſible bodies are found to exiſt;—namely, that of a *ſolid*—that of a *fluid*—and that of an *elaſtic fluid*, or *gas*; and it is well known that every ſubſtance with which we are acquainted—all ponderable matter without exception,—is capable of exiſting alternately under all thoſe forms indifferently; and that the form under which it appears *at any given time* depends on its *temperature at that time.*

We know farther that every identical ſubſtance undergoes theſe different changes of form at certain fixed temperatures: and when we conſider the ſubject with attention we ſhall find that, had not theſe temperatures been *fixed*—and had they not been *different* in *different bodies*, it would have been utterly impoſſible for us to have identified any ſubſtance whatever.

Perhaps this is the only eſſential difference that really exiſts among bodies that appear to us to be different.

But not only the degrees of Heat, or points in the ſcale of temperature, at which the forms of different bodies are changed, are various; but the *extent of the variation of temperature* under which a ſubſtance can preſerve, or continue to maintain its form in its *middle ſtate*,—that of *fluidity*—or rather *liquidity*,—is very different in different bodies: and this laſt circumſtance has a wonderful effect in increaſing the variety of the compoſitions and decompoſitions which are continually taking place in the various operations of nature on the ſurface of the globe.

Another circumſtance, not leſs prolific in events, is the union which takes place between bodies of *different kinds;* and thoſe moſt important changes in regard to the degrees of Heat which the bodies *ſo united* can ſupport without having their forms changed, which are found to reſult from ſuch union.

When, to the eſtabliſhed laws which have been diſcovered in the operations of nature in the change of form in ſubſtances that appear to us to be *ſimple*, we add thoſe which have been found to obtain in the changes of form of bodies that are known to be *compounded*, we ſhall perhaps be able to conceive ſome more diſtinct ideas with regard to the nature of thoſe mechanical operations which take place in chemical proceſſes.—I call them *mechanical*,—for mechanical they muſt of neceſſity be, according to the moſt rigid interpretation of that expreſſion.

But the hypotheſis of the exiſtence of *intenſe Heat* in the midſt of cold liquids is ſo new, and ſeems to be ſo contrary to the reſult of all our experience and obſervation, that I feel it to be neceſſary to take ſome pains to illuſtrate the matter.

And firſt, we muſt not expect always to find traces remaining of the exiſtence of intenſe Heat, even where there are the ſtrongeſt reaſons to think it has actually exiſted; for as often as Heat is diſperſed, or carried off, before it has had time to produce any *changes of form*, or chemical changes or combinations in the bodies to which it is communicated, it leaves no marks behind it.

Fire-arms are often found to miſs fire, even when many live ſparks from the flint and ſteel actually fall into the pan among the priming; but nobody, ſurely, will pretend that the ſmall particles of *red-hot iron* which fall among the grains of the gunpowder, and cool in contact with them, are not intenſely hot;—incomparably more ſo than would be neceſſary to inflame the powder were their Heat of ſufficient *duration* to produce that effect. Had theſe ſmall ſparks been inviſible, it is highly probable that their exiſtence would never have been ſuſpected, and that the fact which they prove would not have been believed.

That gunpowder may be inflamed, it is neceſſary that the ſulphur which conſtitutes one of its component parts ſhould be firſt *melted*, and then *boiled*; for it is the *vapour of boiling ſulphur* which always takes fire when gunpowder is kindled.

Were melted ſulphur a conductor of Heat, there is reaſon to think that gunpowder would be very far from being ſo inflammable as we find it to be.

As thoſe who have not been much accuſtomed to meditate on the ſubject under conſideration may find ſome difficulty in conceiving how it is poſſible for intenſe Heat to be *excited* in, or to *exiſt* in the midſt of a maſs of any cold liquid, as of water for inſtance, without immediately producing viſible effects, I feel it to be my duty to put that matter in the cleareſt light poſſible, and to ſhow that what I have conſidered as being *probable* is moſt undoubtedly very far from being impoſſible.

The beſt method of proceeding in inquiries of this kind, where the principle object is to diſcover whether a ſuppoſed event, which, from its nature, cannot fall under the cognizance of our ſenſes, is, or is not poſſible, ſeems to me to be, to begin by ſuppoſing the event to have actually taken place, and then to trace its neceſſary conſequences, and compare them with thoſe appearances which are actually found to take place.

Adopting this method, we will ſuppoſe a quantity of pure water, at the mean temperature of the atmoſphere in England, that of 55° F., to be put into a clean and very tranſparent glaſs tumbler, placed in a window and expoſed to the direct rays of the ſun. If the glaſs and the water are both *perfectly tranſparent*, it is evident that no Heat will be generated in either of them by the ſun's light.

If now a ſmall particle of any opaque ſolid body be ſuſpended in the midſt of the water in the tumbler; thoſe rays of light, which impinging againſt it, are abſorbed by it, muſt neceſſarily generate

Heat in the very moment when they are ſtopped. This is an incontrovertible fact, which nobody will diſpute.

In order to render this imaginary Experiment more intereſting, we will ſuppoſe the ſolid body put into the water to be a ſmall particle of yellow amber; and that its ſpecific gravity is ſo exactly equal to that of the water that it has no tendency to move in it, either upwards or downwards, and conſequently will remain in the ſituation where it is placed, without being ſuſpended; and we will ſuppoſe farther, that this ſolid particle of amber is nearly globular, and $\frac{1}{1500}$ of an inch in diameter, which is juſt equal to the diameter of a ſingle thread of ſilk, as ſpun by the worm; and is probably one of the ſmalleſt objects that is perceptible by the human eye, unaſſiſted by art.

As it is evident that Heat muſt be generated, or excited, in this ſmall particle of amber, by the light it ſtops or abſorbs, the points which remain to be diſcuſſed are, therefore, what *its intenſity* is at the moment of its exiſtence? and what are the effects which it ought to produce in conſequence of that intenſity?

The reaſons have already been mentioned which render it probable that when heat is generated by the rays of light its intenſity, *where it is generated*—and before it has been diminiſhed in conſequence of its diſperſion, is always the ſame: and taking it for granted that this is the caſe in fact, we will endeavour to trace the operations of that Heat,—

extreme in its intenſity, or degree, but ſmall in regard to its quantity, or to the ſpace it occupies,—which is generated in the particle of amber in the Experiment under conſideration.

As this Heat muſt firſt exiſt *where it is generated*, it is evident that it muſt exiſt at the ſurface of the particle of amber; and as all ſolid bodies are, in a greater or leſs degree, conductors of Heat, a part of this Heat will penetrate the ſubſtance of the ſolid particle, while another part of it will be carried off by the cold particles of water in contact with the ſurface thus heated by the light.

It remains therefore to be determined what the effects are which this Heat ſo abſorbed, on the one hand, by the ſolid particle of amber, and communicated to the water on the other, ought neceſſarily to produce. And firſt, if the diſperſion of the Heat by both theſe means ſhould be ſufficiently rapid to prevent its *accumulation* to ſuch a degree as to melt the amber, it is evident that no viſible effects by which its exiſtence could be diſcovered would be produced in that ſubſtance; and this event—(the fuſion of the amber)—will depend on three circumſtances; namely, *Firſt*, on the temperature at which amber melts;—*Secondly*, on the facility with which Heat expands and is diſperſed in a ſolid maſs of that ſubſtance, or on its conducting power;—and *Thirdly*, on the rapidity with which the Heat generated at the ſurface of the amber is carried off by the cold Fluid in which it is immerſed.

Though I do not think there would be any reaſon for ſurpriſe, even admitting the exiſtence of the ſuppoſed intenſe Heat, ſhould the amber be found not to be melted under the circmmſtances deſcribed; yet it appears to me to be extremely probable, that if amber, in a very fine powder, were mixed with any tranſparent oil, capable of ſupporting a great degree of Heat without being reduced to vapour, and expoſed in it to the direct rays of a very bright ſun, the amber would melt, and be diſſolved, though perhaps very ſlowly.

But if amber does not melt when expoſed in water to the action of the ſun's beams, and conſequently ſuffers no viſible change by which the exiſtence of the Heat ſuppoſed to be generated at its ſurface by the light can be detected, ought not this Heat, were it in fact as intenſe as it is ſuppoſed to be, to produce ſome viſible effects in the water, by which its exiſtence would neceſſarily be diſcovered?

To reſolve this doubt, we muſt inquire *what* viſible effects it would be poſſible for the Heat in queſtion to produce in the water. Now if we ſuppoſe the water not to be decompoſed by this Heat, which, as no chemical change is ſuppoſed to take place in the amber, cannot happen, the only effect this Heat can poſſibly produce on the water is an increaſe of its temperature, which increaſe muſt, however, be much too ſmall to be detected, either by the feeling, or by the thermometer.

It might perhaps be expected that *ſteam* would be found at the heated ſurface of the particle of amber, and become viſible; but when we conſider the matter for a moment, we ſhall ſee that it is quite impoſſible that ſuch an event ſhould happen; for even on the ſuppoſition (which however is far from being probable)—that the ſame individual particles of water which come into contact with the hot ſurface of the amber ſhould remain in contact with it till their temperatures ſhould gradually be raiſed to that point at which water is changed to ſteam; yet, from the extreme rapidity with which ſteam condenſes when in contact with cold water, it is evident that it could not exiſt an inſtant under the circumſtances here ſuppoſed. Indeed we have direct proofs that ſteam cannot exiſt under ſuch circumſtances, by what is found to happen when large maſſes of iron, or ſteel, raiſed to a moſt intenſe heat, in a blaſt furnace, are ſuddenly plunged into cold water, by ſmiths, in tempering edge-tools; for theſe maſſes of red-hot metal may be diſtinctly ſeen to be in actual contact with the cold water; and did not a part of the water, which is decompoſed by the hot iron, make its eſcape in the form of inflammable air, it is not probable that there would be any viſible appearance from which the formation of ſteam could be ſuſpected.

Hence we ſee the *poſſibility* of the exiſtence of *intenſe Heat* in the midſt of a maſs of cold water, or of any other tranſparent liquid, *without producing any viſible effects;* or leaving behind it any traces by which its exiſtence could be ſuſpected.

Let us now conſider a caſe in which this intenſe Heat, though perfectly imperceptible on account of the extreme minuteneſs of the particles of matter in which it exiſts, is capable nevertheleſs of producing very viſible effects. Let us ſuppoſe a ſolution of nitro-muriate of gold, in water, to be expoſed to the action of the ſun's rays. If this ſolution were *perfectly* tranſparent, no Heat could poſſibly be generated in it by light; but as it is not ſo, Heat, in the higheſt degree of intenſity, muſt neceſſarily be generated by thoſe opaque particles (of the oxyd of gold) by which it is ſtopped. Now as gold is a very heavy ſubſtance, it is evident that it muſt be reduced to extremely ſmall particles, in order that, when changed to an oxyd by its union with oxygen, it may be diſſolved in and continued ſuſpended in water; and it is clear that the ſmaller any inſulated particle of matter is, at the ſurface of which Heat is generated in conſequence of the abſorption of light, the more ſuddenly muſt the Heat ſo generated be diſperſed through the whole ſubſtance of the particle, and the more equally and more intenſely muſt that particle be heated: from hence it appears evidently, that if the particles of the oxyd diſperſed about in the water are but *ſmall enough*, the Heat generated in them by the ſun's rays will be ſufficient to expel the oxygen united to the gold, and revive that metal.

There is one very obvious objection, that will doubtleſs be made to this concluſion, which, however, may eaſily be removed. The particle of the metallic oxyd which is ſuppoſed to be heated, is in contact with the water; how does it happen that a great part of this Heat does not immediately paſs off into that cold Fluid? I might anſwer, becauſe both water and ſteam are non-conductors of Heat; —and might adduce in ſupport of this reaſon the well known fact, that a drop of water dropped on a piece of iron, heated to moſt intenſe white Heat, will remain ſome time on the iron without being evaporated, even conſiderably longer than if the iron were much leſs hot;—but a circumſtance attending the beautiful Experiment in which iron is burned in oxygen gas, affords a more direct proof of the fact in queſtion.

As this Experiment is commonly made, the iron, which is a piece of ſmall wire, a few inches long, is introduced into a bottle, with a narrow neck, which contains the oxygen gas; the wire being fixed in its place, by cauſing its upper end to paſs through a cork ſtopple, which is fitted to the mouth of the bottle. The lower end of the wire is pointed; and it is ſet on fire by being firſt heated in the flame of a candle, and then plunged ſuddenly, while red-hot, into the bottle. The combuſtion begins the moment the end of the wire enters the oxygen gas; and the metal continues to burn with the utmoſt violence, and with a copious emiſſion of intenſe white light, till the wire, or till all the gas is conſumed, affording one of the moſt brilliant and moſt intereſting ſights that can be imagined.

The product of this combuſtion is the oxygenation of the iron; and this metallic oxyd, in a ſtate of fuſion, and heated to the moſt intenſe white Heat, falls to the bottom of the bottle in globules of different ſizes.

To protect the glaſs againſt theſe drops of calx of iron in fuſion, it is uſual to leave a quantity of cold water in the bottle, enough, for inſtance, to cover its bottom to the height of about an inch: but I have frequently ſeen numbers of theſe globules, much ſmaller than peas, which have not only deſcended *red-hot* through the water; but have remained red-hot at the bottom of the bottle, ſurrounded by the water, at leaſt two or three ſeconds; and actually melted the glaſs on which they repoſed, (and as far as I can recollect,) without producing the ſmalleſt appearance of ſteam.

The water could not be decompoſed, for the iron was already ſaturated with oxygen.

This Experiment will, I fancy, be conſidered as affording an indiſputable proof that *intenſe Heat* may exiſt, at leaſt for a ſhort time, in a ſmall particle of matter ſurrounded by a cold Fluid.

Now, as it has been found by actual Experiment, that when a ſolution of nitro-muriate of gold in water is expoſed to the action of the ſun's rays, the gold is revived; and as it is known that an oxyd of gold may be reduced in the dry way, without addition, or merely by intenſe Heat, why ſhould we not conclude that it is merely by *Heat* that that metal is revived in the caſe under conſideration,—and that the *intenſity* of the Heat by which this oxygenation is effected, is preciſely the ſame in both caſes?

Should this ſuppoſition be admitted, we might, perhaps, venture to proceed one ſtep farther, and conſider the nature and progreſs of the mechanical operations which take place in diſoxygenation of metals, or their precipitation from a ſolution of their oxyds, when that operation is effected by means of Heat generated,—not by light,—but by the contact or union of infinitely ſmall particles of bodies, different in kind, and diſpoſed to generate or to abſorb ſenſible Heat on coming together; which particles being diſperſed about in the liquid ſolution, and in the ſubſtance added to it to effect the precipitation, are by this mixture brought into contact.

This would naturally lead us to an examination of the phenomena of ſolution,—and thoſe clearly underſtood would, no doubt, give us a diſtinct view of the mechanical operations by which thoſe tendencies to union are effected, which have been deſignated under the name *elective attraction.*

But how arduous an undertaking! what intenſe ſtudy!—what efforts of the imagination would be neceſſary to trace out and form diſtinct ideas of ſuch a ſucceſſion of events, all perfectly imperceptible by our organs, though aſſiſted by all the reſources of art!

Senſible of my own weakneſs, I dare not proceed any farther.—Perhaps it will be thought that I have already advanced much too far;—but it is right that I ſhould acknowledge fairly, that in the preſent caſe, the temerity I have ſhown has not been entirely without deſign.

There are *two* ways in which philoſophers, as well as other men, may be excited to action; and induced to engage zealouſly in the inveſtigation of any curious ſubject of inquiry:—they may be *enticed*,—and they may be *provoked.*

It will probably not eſcape the penetration of my reader, that I have endeavoured to uſe both theſe methods.—I am well aware of the danger that attends the latter of them; but the paſſionate fondneſs that I feel for the favourite objects of my purſuits frequently hurries me on far beyond the bounds which prudence would mark to circumſcribe my adventurous excurſions.

CHAP. IV.

An Account of a Variety of Miſcellaneous Experiments.—Thermometers with cylindrical Bulbs may be uſed to ſhow that Liquids are Non-conductors of Heat.—Ice-cold Water may be heated and made to boil ſtanding on Ice.—Remarkable Appearances attending the thawing of Ice, and the melting of Tallow, and of Bees'-Wax, by means of the radiant Heat projected downwards by a red-hot Bullet.—Beautiful Cryſtals of Sea-Salt formed in Brine ſtanding on Mercury.—Olive Oil ſoon rendered colourleſs by Expoſure to the Air ſtanding on Brine.—An Attempt to cauſe radiant Heat from a red-hot Iron Bullet to deſcend in Oil.—Account of an artificial Atmoſphere in which horizontal Currents were produced by Heat.—Conjectures reſpecting the proximate Cauſes of the Winds.

THOUGH this Eſſay is already grown to a much larger ſize than I originally intended, and even larger than I could have wiſhed;—(well knowing how great an evil a great Book is generally thought to be;)—I could not bring it to a concluſion without adding one Chapter more. In this Chapter the reader will find accounts of ſeveral Experiments, ſome of which he will probably conſider as not altogether unintereſting. To take up as little of his time as poſſible I ſhall be very brief in theſe accounts, and in general ſhall leave the reader to draw his own concluſions from the reſults of the Experiments I ſhall deſcribe.

§ 1. *An Account of ſeveral ſimple Experiments, which ſhow that Heat does not deſcend in Fluids.*

If a thermometer conſtructed with a long and narrow, naked cylindrical bulb,—(6 inches long, for inſtance, and ½ an inch in diameter,)—and filled with mercury, oil, ſpirits of wine, or any other Fluid proper for that purpoſe, with which it is required to make the Experiment in queſtion; ſuch thermometer being at the temperature of the air in ſummer, or at any temperature above the point of freezing water, if the lower end, or half, of its bulb be plunged into a glaſs tumbler filled quite full to the brim with pounded ice and water, the height of the Fluid in the tube of the inſtrument will ſhow that *half the Fluid in the cylindrical bulb of the inſtrument is ice-cold*, while the temperature of the other half of it remains unchanged.

The reſult will be the ſame, when, to prevent the communication of Heat from the air during the Experiment, that part of the bulb of the thermometer (the ſuperior half of it) which projects above the level of the top of the tumbler is covered with a ſheath lined with ſoft fur.

When more or leſs than half of the bulb of the thermometer is plunged into the ice and water, the height of the liquid in the tube of the inſtrument will ſhow that that part only of the Fluid in the bulb is cooled which occupies the part of the bulb that is immerſed in the ice and water.

§ 2. *Ice-cold Water, ſtanding on Ice, may be heated and made to boil without melting the Ice, contrary to an Opinion that has generally prevailed.*

Take a thin glaſs tube, 1 inch in diameter, and about 8 or 10 inches long, containing about two or three inches of water, and by plunging the end of the tube into a freezing mixture of pounded ice and ſea-ſalt cauſe the water in the tube to congeal: this being done, pour two or three inches of ice-cold water on the ice; and wrapping up about two inches of the lower end of the tube with a piece of flannel, and holding it inclined at an angle of about 45°; by that part of it which is ſo covered, bring that part of the tube which is at the height of the ſurface of the Fluid-water to be juſt over the point of the flame of a burning candle, and diſtant from it about two or three inches. When the water in that part of the tube begins to boil, the tube may be advanced ſlowly over the flame of the candle; and if due care be taken to prevent a too ſudden application of the Heat, all the water in the tube to within one quarter of an inch of the ice may be brought into the moſt violent ebullition before the ice will begin to be melted, and at laſt will appear to boil even at the very ſurface of the ice.

§ 3.

§ 3. *The radiant Heat from a red-hot Iron Bullet does not appear to be able to make its Way downwards through liquid Water, nor through melted Tallow, nor melted Wax.*

1ſt Experiment.—A very ſmall mercurial thermometer, with a naked globular bulb, was laid down in an horizontal poſition on two ſmall projections of wax, in the bottom of a ſhallow wooden diſh, in ſuch a manner that the engraved ſide of the ſcale of the thermometer lying uppermoſt, the height of the mercury in its tube could be obſerved. This being done, I poured cold water into the diſh till it ſtood at the height of about ¼ of an inch above the bulb of the thermometer, and then preſented to the thermometer an iron bullet about 1½ inches in diameter, red-hot, which I held (by means of a fit handle) directly over its bulb at the diſtance of about an inch.

The thermometer ſeemed to take very little notice of the vicinity of the red-hot iron.

When its bulb was covered with oil the reſult of the Experiment was much the ſame; but when it was expoſed naked, or uncovered by a liquid, to the rays from the hot iron, it appeared to acquire Heat very rapidly. But the two following Experiments were ſtill more deciſive and ſatisfactory.

2d Experiment.—A ſhallow earthen diſh, about 3 inches deep and 12 inches in diameter at its brim, was filled with water, and being expoſed in a cold room in winter, the water was frozen, and formed a cake a cake of ice at its ſurface, about an inch thick. Letting the diſh remain in its place, in order that the ſurface of the ice might remain perfectly horizontal, (which was neceſſary to the complete ſucceſs of the Experiment, as will preſently be ſeen,) I entered the room with a chafing-diſh filled with live coals, in the midſt of which was my iron bullet, perfectly red-hot; and taking out the bullet from among thoſe burning coals, I held it over the centre of this horizontal ſheet of ice, and diſtant from it about $\frac{1}{10}$ of an inch.

The ice directly under the red-hot bullet was ſoon thawed; but the depth to which it was thawed was very inconſiderable: the water, however, extended itſelf ſlowly from the centre towards the circumference, and at length a circular ſpot 2 or 3 inches in diameter in the centre of the ſurface of the ice was covered with it, though but to a very inconſiderable depth.

This little ſpreading ſea appeared to prey on the wall of ice by which it was ſurrounded on every ſide.

The particles of water in contact with this wall, being rendered ſpecifically lighter on becoming ice-cold, they move upwards, and making way for other warmer particles to advance from below, cauſe currents in oppoſite directions to ſet between the centre (where the hot-iron remains) and the circumference.—As a current at the temperature of 41° muſt neceſſarily ſet downwards at the middle of the circle, this current ſtriking againſt the middle of the excavation formed in the ice ought to deepen it gradually in that part, though but ſlowly,—and this is what was actually found to be the caſe; for the bottom of this excavation was not perfectly flat, but was deeper at and near its centre than at its ſides.

3d Experiment.—When this Experiment was varied by uſing a flat cake of tallow inſtead of a cake of ice, a very extraordinary appearance indeed preſented itſelf, which at firſt ſurpriſed me very much, but which I ſoon perceived was a new, and very ſtriking proof that Fluids are non-conductors of Heat.

The bottom of the circular cavity in the cake of tallow which was occupied by that part of the tallow that had been melted in the Experiment, inſtead of being concave, as I had found that in the ice to be,—or flat,—as I expected to find this,—was *convex* in the middle, or rather roſe up in the form of a protuberance, or very blunt point, the extremity of which reached almoſt to the ſurface of the melted tallow! As the iron bullet was held as near as poſſible to the tallow, the end of this projection, which remained unmelted, was certainly not more than $\frac{2}{10}$ of an inch diſtant from this red-hot ball! Reflecting on the unexpected reſult of this Experiment I was much ſtruck, and not a little humiliated, with the proof it ſeemed to me to afford of the impoſſibility of predicting with certainty any event, however inevitable it may appear, which has not actually been ſeen to happen.

Though I well know how the Heat muſt be communicated under the given circumſtances, and could foretell with certainty the directions of the currents it muſt neceſſarily occaſion in the melting tallow; yet, the utmoſt efforts of my intellectual powers, exerciſed as they were by much meditation, were not ſufficient to enable me to foreſee that the point where leaſt Heat would be communicated was that preciſely which was neareſt to the red-hot bullet; and that a protuberance of unmelted tallow would be left in that place.

Let thoſe be very cautious who ſpeculate on the ſuppoſed reſults of Experiments they have never made!

On repeating this Experiment, and varying it by uſing a cake of fine bleached *bees-wax*, inſtead of tallow, the reſult was much the ſame: the protuberance, however, in the middle of the circular cavity occupied by the melted wax, though perfectly perceptible, was leſs conſiderable, in height, than that in the cake of tallow.

§ 4. *Beautiful Cryſtals of Sea-Salt formed in Brine ſtanding on Mercury.*

A ſmall quantity of ſtrong brine, ſtanding on mercury in an open glaſs tumbler, having by accident been left in a room in a retired part of the houſe, I obſerved at the end of about ſix months, that two beautiful cryſtals of ſalt, perfectly quadrangular, had been formed in it, one of which was $\frac{14}{40}$ of an inch long, $\frac{11}{40}$ of an inch wide, and $\frac{5}{40}$ of an inch in thickneſs; and the other $\frac{15}{40}$ of an inch long, $\frac{10}{40}$ of an inch wide, and $\frac{11}{80}$ of an inch thick.

Did the Fluid mercury on which this brine repoſed contribute?—and how?—to the regularity of the form, and the uncommon ſize of theſe cryſtals?—And might not beautiful cryſtals of other ſalts be procured by ſimilar means?

§ 5. *Olive Oil rendered colourleſs by Expoſure to the Air ſtanding on Brine.*

A quantity of *olive oil*, about ¼ of an inch in depth, having by accident been left ſtanding in an open glaſs jar, about four inches in diameter, on about a quart of brine, moderately ſtrong, in a retired room, where the ſun's rays never enter; at the end of about ſix months I obſerved that the oil had become perfectly colourleſs, and appeared to me to be nearly as tranſparent as the pureſt water. On the approach of winter I found that this oil was much more liable to be congealed with cold than oil of the ſame kind which had ſtood near it many months in a large glaſs bottle cloſed with a cork.

§ 6. *An unſucceſsful Attempt to cauſe radiant Heat from a red-hot Iron Bullet to deſcend in Oil.*

Having poured a quantity of this colourleſs oil into a glaſs tumbler, and cauſed it to congeal throughout, I preſented to its upper ſurface a red-hot iron bullet, 1½ inches in diameter, and held it quite cloſe to the oil, ſeveral minutes, till the bullet ceaſed to be red-hot. As the oil ſeemed rather to be merely thickened by the cold, and to have loſt its tranſparency in conſequence of the preſence of a number of opaque particles, which were everywhere diſperſed about in it, than to be congealed into a ſolid maſs, I thought that if it were poſſible for radiant Heat to deſcend in any Fluid it might perhaps be in this; and if this ſhould happen, I was certain to make the diſcovery by the manner in which the oil recovered its tranſparency; for ſhould radiant Heat deſcend, the form of the maſs of oil firſt reſtored to its tranſparency muſt neceſſarily have been *hemiſpherical*, or ſome ſection of a ſphere, or at leaſt of ſome convex figure: but the under part of that part of the oil which was reſtored to its tranſparency in this Experiment was, to all appearance, as perfectly flat and horizontal as the upper ſurface of it, which proves that the Heat, by which the congealed oil was thawed, was communicated to it,—not immediately by the red-hot bullet,—but *mediately* by means of the Heat abſorbed by or generated in the ſides of the tumbler. This Experiment appears to me to be important in many reſpects; but it would be foreign to my preſent purpoſe to engage in an inveſtigation of the ſubject with which it is moſt intimately connected.

I cannot finiſh this Eſſay without giving my reader an account of one more Experiment, the reſult of which was not only quite unexpected, but uncommonly intereſting.

Happening accidentally to place in a window the little inſtrument I had contrived for rendering viſible the internal motions which are occaſioned in water when Heat is propagated in that Fluid*, as it was winter, and the room was warmed by a German ſtove, that ſide of the inſtrument which happened to be neareſt the window being expoſed to a current of cold air, while the inſtrument received Heat continually on the other ſide from the warmer air of the room, the liquid in the inſtrument was thrown into motions which never ceaſed, and afforded a very intereſting ſight.

With a view merely to amuſe myſelf, and the friends who ſhould happen to call in to viſit me,—and without the ſmalleſt expectation of making any new diſcoveries,—I contrived, and cauſed to be executed, the inſtrument I am now about to deſcribe, which I thought could not fail to render theſe motions perpetual, and exhibit them in a ſtriking manner.

A flat box was formed of two equal panes, each 13 inches high, and 10½ inches wide, of fine ground glaſs, fitted into a ſquare frame of braſs in ſuch a manner, that theſe two panes (which are parallel to each other) are at the diſtance of 1 inch from each other. In the middle of the top of this braſs frame there is a circular opening about ½ an inch in diameter, into which a projecting cylindrical braſs tube, about half an inch in length, is ſoldered; and in the middle of the bottom of the frame there is a ſimilar tube which projects downward. The firſt of theſe openings ſerves for introducing into the flat box the liquid with which it is filled; and the other for drawing it off; and they are both well cloſed with fit ſtopples of cork.

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* For a deſcription of this inſtrument ſee Chapter II. of Part I. of this Eſſay.

On both ſides of this braſs frame there are deep grooves into which the panes of glaſs are fitted, and the box was made water-tight by luting the joinings of the glaſs with the frame with glaziers putty. On the outſide of the frame there are thin projections of ſheet braſs, by means of which the box was fixed in one of the ſaſhes of a window in my room, where it occupied the place of a pane of glaſs, which was removed to make way for it. This window fronts the ſouth-eaſt, and conſequently is expoſed to the ſun a great part of the day.

Having provided a ſufficient quantity of the ſaline ſolution,—(of the ſame kind as was uſed in conſtructing the inſtrument above mentioned, contrived for rendering viſible the internal motions in Fluids) and having mixed with it a due proportion of pulverized yellow amber, I now filled the box half full with this mixture; and as the air in the room was conſiderably warmer than that without, I expected that the motions in the liquid occaſioned by the paſſage of the Heat would immediately commence.

This actually happened;—but how great was my ſurpriſe, when, inſtead of the vertical currents I expected, I diſcovered horizontal currents, running in oppoſite directions,—one above another,—or regular WINDS,—which, ſpringing up in the different regions of this artificial atmoſphere, prevailed for a long time with the utmoſt regularity;—while the ſmall particles of the amber collecting themſelves together, formed clouds of the moſt fantaſtic forms, which being carried by the winds, rendered the ſcene perfectly faſcinating!

It would be impoſſible to deſcribe the avidity with which I gazed on theſe enchanting appearances.

In the ſtate of enthuſiaſm I then was, it really ſeemed to me that Nature had for a moment drawn back the veil with which ſhe hides from mortal eyes her moſt ſecret and moſt intereſting operations;—and that I now ſaw the machinery at work by which winds and ſtorms are raiſed in the atmoſphere!

Nothing ſeemed to be wanting to complete this bewitching ſcene, and give it the air of perfect enchantment, but that lightning, in miniature, ſhould burſt from theſe little clouds: and they were frequently ſo thickened up, and had ſo much the appearance of preparing for a ſtorm, that had that event actually taken place, it could hardly have increaſed my wonder and ecſtaſy.

There were ſeveral accidental circumſtances attending this Experiment, which contributed to render it more intereſting. The ſun, which happened to be remarkably bright, ſhone full upon the window where the apparatus was placed; and as the grooves in the frame in which the plates of glaſs were fixed were not deep, that part of this frame which formed the narrow bottom of the box being expoſed to the ſun's rays, a conſiderable quantity of Heat was generated by them in that place, as appeared by the motions of the particles of pulverized amber which lay on the bottom of the box, or thoſe which were brought there by the currents

When theſe particles, on being heated by the ſun-beams, began to move, they firſt aroſe up nearly perpendicularly; but before they had riſen to any conſiderable height, they were carried away obliquely, and nearly in an horizontal direction, by the lower current, anſwering to the wind which, in the atmoſphere, prevails at the ſurface of the earth.

The perpendicular riſe of theſe particles from the bottom of the box, and the ſubſequent change of their direction, called to my remembrance an appearance very common in hot countries, which I recollected to have often ſeen, and by which I had often been amuſed in my youth: in very hot and dry weather, when the wind is ſtill and the ſun very powerful, the air which lies on the ground often appears in the moſt violent agitation, reſembling that of a boiling liquid; which motion is moſt rapid at the ſurface of the earth, and appears to ceaſe at the height of five or ſix feet above the ground.

Is not this violent agitation occaſioned by the conflict which takes place between the hot and the comparatively cold air moving *vertically*, and in oppoſite directions, very near the ſurface of the ground? And are not the winds which prevail above, occaſioned by the efforts of whole *ſtrata* of air to aſcend or deſcend obliquely?

The currents I obſerved to prevail in my artificial atmoſphere were never perfectly horizontal; and if my ſuſpicions with reſpect to the cauſe of the winds are well founded, neither can thoſe winds be horizontal which prevail in the ſuperior regions of the atmoſphere of the earth, though they may be very nearly ſo.

The greateſt velocity of the currents in the ſaline liquid in this Experiment was nearly two inches in a minute, but their motions were in general much ſlower. As the windows in the room in which this Experiment was made are double, (as are all thoſe both in ſummer and winter in the apartment I inhabit,) and as the apparatus above deſcribed occupied the place of a pane of glaſs belonging to the inſide window, it was in my power, by opening either the inſide window or the outſide window, to cauſe the Heat on the two oppoſite ſides of the box to be either equal or unequal at pleaſure; and by variations which that arrangement enabled me to make in the Experiments I produced ſeveral intereſting appearances.

There was one very ſtriking appearance indeed, which never failed to preſent itſelf regularly every day during the three weeks that the Experiment was continued *. The clouds, after having been driven about all day by the different currents in the liquid, (of which there were ſometimes as many as ſix or ſeven, running in oppoſite directions at the ſame time,) never failed to collect themſelves together in the evening, into large maſſes; ſometimes forming only one, and ſometimes two or three *ſtrata* at different heights, where they remained, to all appearance perfectly motionleſs, during the night.

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* An end was put to the Experiment by an accident; the box being broken by the careleſſneſs of a ſervant in ſhutting the window-ſhutter.

There can be no queſtion with reſpect to the *proximate* cauſe of this phænomenon: for it was undoubtedly owing to a diminution or total ceſſation of the operation of that cauſe,—of thoſe cauſes,—or of ſome of them,—by which an inequality of temperature in the liquid was produced and continued;—but it would be highly curious to inveſtigate the more remote cauſes of this appearance, and ſee how far *light*, or rather the abſence of it, was concerned in producing it: but that diſcuſſion would lead me into a very abſtruſe inquiry,—that reſpecting *radiant Heat*,—which would take up more time than I am at preſent able to beſtow on it. Perhaps I may find leiſure and courage at ſome future period to attempt that moſt difficult inveſtigation. My reader will doubtleſs have obſerved that I have hitherto *taken pains* to avoid it.

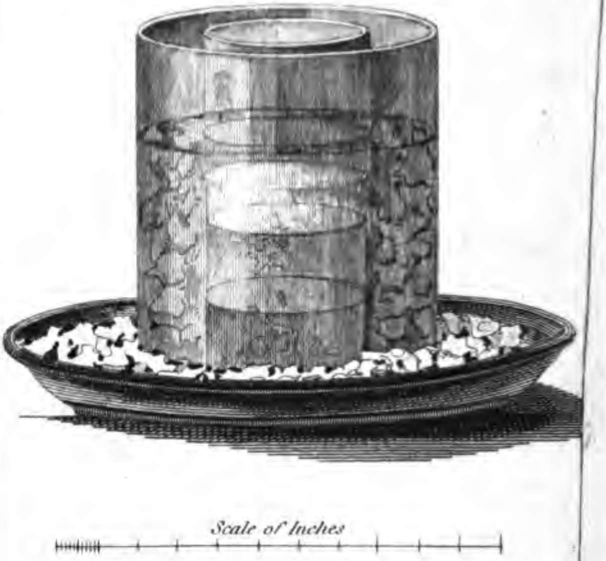
I cannot take my leave of the Experiment I have been deſcribing without giving my reader a faithful account of every thing I can recollect reſpecting it; and particularly of one accidental circumſtance, which, it is poſſible, may have had ſome ſhare in producing the intereſting appearances which ſo powerfully attracted my attention.

The ſaline liquor and the pulverized amber were mixed in a bottle, and were not put into the flat box till after it had been fixed in the ſaſh or frame of the window; but when I came to pour this mixture into the box I found that I had not provided enough of it. To ſupply this defect, without the trouble of emptying the box, I added, at ſeveral different times, pure water, and a ſtrong ſolution of pot-aſh, in ſuch proportions as I knew to be proper to produce the ſpecific gravity required; and then endeavoured to mix the whole as intimately as poſſible by agitating the liquor for ſome conſiderable time by means of a long and ſtrong quill, the end of which I thruſt down into the box through the hole by which the liquor was introduced.

Whether thoſe different portions of liquid were in fact intimately mingled by theſe means, I cannot poſitively determine. They certainly had every appearance of being ſo; for the amber was evidently well mixed, and very equally diſtributed in every part of the Fluid. But even ſhould we grant that the liquid remained divided in different *ſtrata*, arranged according to the ſpecific gravities of the different portions of it that were poured into the box at different times, it does not appear to me that the reſult of the Experiment would be leſs intereſting on that account, or the application of it leſs ſatisfactory in explaining the cauſe of the winds in the atmoſphere.

I am, however, far from being deſirous that much ſtreſs ſhould be laid on this ſingle Experiment, being perfectly ſenſible that others may be contrived, the reſults of which would be more deciſive: in the mean time it appears to me that the hint given us is too plain not to deſerve ſome attention. If it ſhould awaken the curioſity of experimental philoſophers, and excite them to farther inveſtigation, the end I had principally in view in publiſhing this account of it will be completely anſwered.

Fig. 5.



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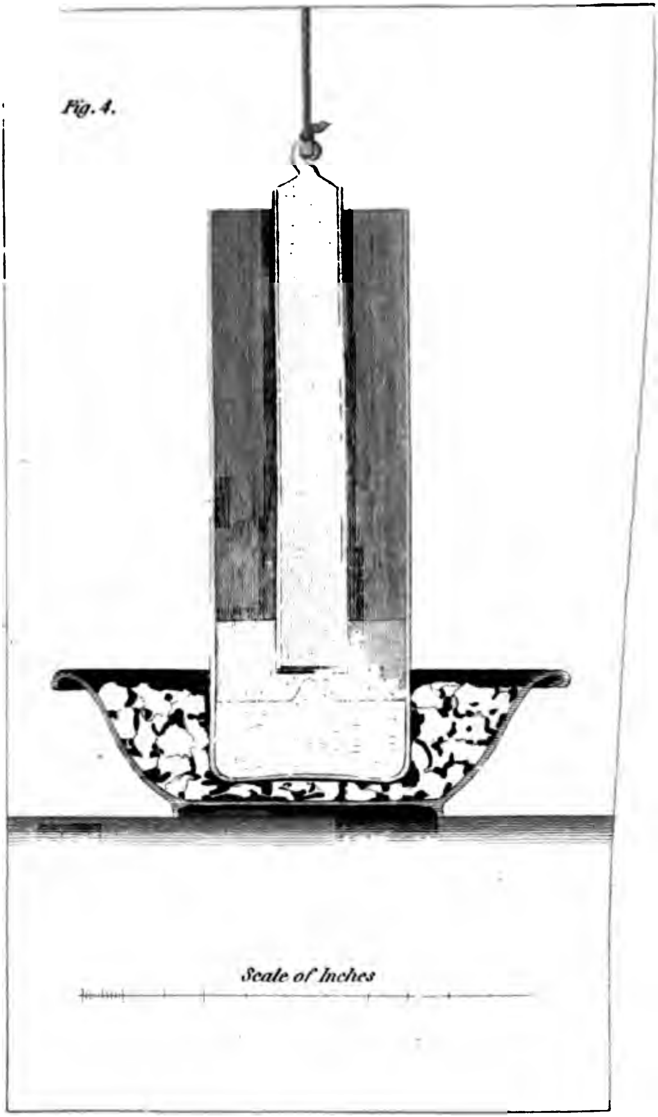
London Pub.d May 1st 1798, by Cadell & Davies Strand.

Plate 3.

Fig. 4.



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DESCRIPTION OF THE PLATES.

PLATE III.

FIG. 4. This Figure repreſents a vertical ſection of the apparatus uſed in the Experiment No. 55. (ſee page 315), in which an attempt was made to melt the top of a projecting point of ice by Heat tranſmitted *downwards* through olive-oil communicated by a ſolid cylinder of iron, heated in boiling water.

In this Figure the tall glaſs jar (in the bottom of which the cake of ice was frozen) is ſtanding in an earthen pan filled with pounded ice.

The *oil* is alſo repreſented ſtanding on the *cake of ice* in the jar; and the iron cylinder in its ſheath of paper ſuſpended in the axis of the jar in ſuch a manner that the lower end of this cylinder, which is flat, is directly over the pointed projection of ice, and diſtant from it $\frac{2}{10}$ of an inch.

PLATE IV.

Fig. 5. This Figure ſhows the manner in which the Experiment No. 57. (ſee page 326) was made, when pure or freſh water in a glaſs jar was made

to repoſe on brine, or water ſaturated with ſea-ſalt, without mixing with it.

In this Experiment the ſmaller jar, which contained the brine, the pure water, and a quantity of olive-oil by which the ſurface of the pure water was covered, ſtood in a larger glaſs jar,—which laſt ſtood in a ſhallow earthen diſh filled with pounded ice and water.

The ſpace between the outſide of the ſmaller jar and the inſide of the larger jar was filled, to the height of about an inch above the level of the ſurface of the oil in the ſmaller jar, with pieces of ice nearly as large as walnuts, and ice-cold water.

THE END OF PART II. OF ESSAY VII.

ESSAY VIII.

OF THE PROPAGATION OF HEAT IN *VARIOUS SUBSTANCES:*

BEING

An Account of a Number of NEW EXPERIMENTS made with a View to the Inveſtigation of the CAUSES of the WARMTH of NATURAL and ARTIFICIAL CLOTHING.

FIRST PUBLISHED IN THE PHILOSOPHICAL TRANSACTIONS.

INTRODUCTION.

THIS Eſſay contains nothing that will be new to philoſophical readers; for it is little more than the ſubſtance of two papers which have already appeared in the Philoſophical Tranſactions of the Royal Society of London; one in the year 1786; and the other (for which the Author had the honour to receive from the Society the Copleian Annual Medal) in the year 1792.

As reference has frequently been made to theſe Papers in ſeveral of the preceding Eſſays; and as many of the Experiments of which an account is given in them are not only intereſting in themſelves, but are neceſſary to be known in all their details in order to judge of ſeveral important concluſions that have been founded on their reſults, the Author has thought that it would not be improper to republiſh them under the preſent form. He was alſo deſirous of adding the ſubſtance of thoſe Papers to his Sixth and Seventh Eſſays, in order that all that he has written on the *Science* of *Heat* might be brought together in one volume.

The Eſſays which are deſtined to compoſe the next volume (many of which are already in great forwardneſs) are all on practical ſubjects of a popular nature, and of general utility; and on that account it was judged beſt to keep them ſeparate from thoſe contained in this volume, which partake more of the nature of abſtruſe philoſophical inveſtigations.

Various unforeſeen events have contributed to retard the publication of the promiſed Eſſays on Kitchen Fire-places—on Cottage Fire-places—and on Clothing; but the Author has well-founded hopes of being able to bring them forward in the courſe of a few months.

ESSAY VIII.

OF THE PROPAGATION OF HEAT IN VARIOUS SUBSTANCES.

CHAP. I.

An Account of the Inſtruments that were prepared for making the propoſed Experiments.—A Thermometer is conſtructed whoſe Bulb is ſurrounded by a TORRICELLIAN VACUUM.—*Heat is found to paſs in a Torricellian Vacuum with greater Difficulty than in Air.—Relative conducting Powers of a Torricellian Vacuum and of Air with regard to Heat determined by Experiment.—Relative conducting Powers of dry Air and of moiſt Air.—Relative conducting Powers of Air of different Degrees of Denſity.—Relative conducting Powers of* MERCURY; WATER; AIR; *and a* TORRICELLIAN VACUUM.

[Read before the ROYAL SOCIETY, March 9, 1786.]

EXAMINING the conducting power of air, and of various other fluid and ſolid bodies, with regard to Heat, I was led to examine the conducting power of the *Torricellian vacuum.* From the ſtriking analogy between the electric fluid and Heat reſpecting their conductors and non-conductors, (having found that bodies, in general, which are conductors of the electric fluid, are likewiſe good conductors of Heat, and, on the contrary, that electric bodies, or ſuch as are bad conductors of the electric fluid, are likewiſe bad conductors of Heat,) I was led to imagine that the Torricellian vacuum, which is known to afford ſo ready a paſſage to the electric fluid, would alſo have afforded a ready paſſage to Heat.

The common experiments of heating and cooling bodies under the receiver of an air-pump I conceived to be inadequate to determining this queſtion; not only on account of the impoſſibility of making a perfect void of air by means of the pump; but alſo on account of the moiſt vapour, which exhaling from the wet leather and the oil uſed in the machine, expands under the receiver, and fills it with a watery fluid, which, though extremely rare, is yet capable of conducting a great deal of Heat: I had recourſe therefore to other contrivances.

I took a thermometer, unfilled, the diameter of whoſe bulb (which was globular) was juſt half an inch, Paris meaſure, and fixed it in the center of a hollow glaſs ball of the diameter of 1¼ Paris inch, in ſuch a manner, that the ſhort neck or opening of the ball being ſoldered faſt to the tube of the thermometer 7½ lines above its bulb, the bulb of the thermometer remained fixed in the center of the ball, and conſequently was cut off from all communication with the external air. In the bottom of the glaſs ball was fixed a ſmall hollow tube or point, which projecting outwards was ſoldered to the end of a common barometer tube about 32 inches in length, and by means of this opening the ſpace between the internal ſurface of the glaſs ball and the bulb of the thermometer was filled with hot mercury, which had been previouſly freed of air and moiſture by boiling. The ball, and alſo the barometrical tube attached to it, being filled with mercury, the tube was carefully inverted, and its open end placed in a bowl in which there was a quantity of mercury. The inſtrument now became a barometer, and the mercury deſcending from the ball (which was now uppermoſt) left the ſpace ſurrounding the bulb of the thermometer free of air. The mercury having totally quitted the glaſs ball, and having ſunk in the tube to the height of 28 inches, (being the height of the mercury in the common barometer at that time), with a lamp and a blow-pipe I melted the tube together, or ſealed it hermetically, about three quarters of an inch below the ball, and cutting it at this place with a fine file, I ſeparated the ball from the long barometrical tube. The thermometer being afterwards filled with mercury in the common way, and furniſhed with a ſcale, I now poſſeſſed a thermometer whoſe bulb was confined in the center of a *Torricellian vacuum*, and which ſerved at the ſame time as the body to be heated, and as the inſtrument for meaſuring the Heat communicated.

Experiment, N° 1.

With this inſtrument (ſee Fig. 1.) I made the following Experiment. Having plunged it into a veſſel filled with water, warm to the 18th degree of REAUMUR's ſcale, and ſuffered it to remain there till it had acquired the temperature of the water, that is to ſay, till the mercury in the incloſed thermometer ſtood at 18°, I took it out of this veſſel and plunged it ſuddenly into a veſſel of boiling water, and holding it in the water (which was kept conſtantly boiling) by the end of the tube, in ſuch a manner that the glaſs ball, in the center of which was the bulb of the thermometer, was juſt ſubmerged, I obſerved the number of degrees to which the mercury in the thermometer had ariſen at different periods of time, counted from the moment of its immerſion. Thus, after it had remained in the boiling water 1 min. 30 ſec. I found the mercury had riſen from 18° to 27°. *After* 4 minutes had elapſed, it had riſen to 44°$\frac{9}{10}$; and at the end of 5 minutes it had riſen to 48°$\frac{2}{10}$.

Experiment, N° 2.

Taking it now out of the boiling water I *ſuffered* it to cool gradually in the air, and after it had acquired the temperature of the atmoſphere, which was that of 15° R. (the weather being perfectly fine) I broke off a little piece from the point of the ſmall tube which remained at the bottom of the glaſs ball, where it had been hermetically ſealed, and of courſe the atmoſpheric air ruſhed immediately into the ball. The ball ſurrounding the bulb of the thermometer being now filled with air, (inſtead of being emptied of air, as it was in the before-mentioned Experiment,) I reſealed the end of the ſmall tube at the bottom of the glaſs ball hermetically, and by that means cut off all communication between the air confined in the ball and the external air; and with the inſtrument ſo prepared I repeated the Experiment before-mentioned; that is to ſay, I put it into water warmed to 18°, and when it had acquired the temperature of the water, I plunged it into boiling water, and obſerved the times of the aſcent of the mercury in the thermometer. They were as follows:

	Time elapſed.	Heat acquired.
Heat at the moment of being plunged into the boiling water, - - - -		18° R.
	M. S.	
After having remained in the boiling water	0 45	27
	1 0	$34\frac{4}{10}$
	2 10	$44\frac{9}{10}$
	2 40	$48\frac{2}{10}$
	4 0	$56\frac{2}{10}$
	5 0	$60\frac{9}{10}$

From the reſult of theſe Experiments it appears evidently, that the Torricellian vacuum, which affords ſo ready a paſſage to the electric fluid, ſo far from being a good conductor of Heat, is a much worſe conductor of it than common air, which of itſelf is reckoned among the worſt: for in the laſt Experiment, when the bulb of the thermometer was ſurrounded with air, and the inſtrument was plunged into boiling water, the mercury roſe from 18° to 27° in 45 ſeconds; but in the former Experiment, when it was ſurrounded by a Torricellian vacuum, it required to remain in the boiling water 1 minute 30 ſeconds = 90 ſeconds, to acquire that degree of heat. In the vacuum it required 5 minutes to riſe 48°$\frac{2}{10}$; but in air it roſe to that height in 2 minutes 40 ſeconds; and the proportion of the times in the other obſervations is nearly the ſame, as will appear by the following Table.

	The bulb of the thermometer placed in the center of the glaſs ball, and			
	ſurrounded by a Torricellian vacuum. (Exp. N° 1.)		ſurrounded by air. (Exp. N° 2.)	
	Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
Upon being plunged into boiling water - -		18°		18°
	M. S.	°	M. S.	°
After remaining in it	1 30	27	0 45	27
	——	——	1 0	$30\frac{4}{10}$
	4 0	$44\frac{9}{10}$	2 10	$44\frac{9}{10}$
	5 0	$48\frac{2}{10}$	2 40	$48\frac{2}{10}$
	——	——	4 0	$56\frac{2}{10}$
	——	——	5 0	$60\frac{0}{10}$

Theſe Experiments were made at Manheim, upon the firſt day of July 1785, in the preſence of Profeſſor Hemmer, of the Electoral Academy of Sciences of Manheim, and Charles Artaria, meteorological inſtrument maker to the academy, by whom I was aſſiſted in making them.

Finding the conſtruction of the inſtrument made uſe of in theſe Experiments attended with much trouble and riſk, on account of the difficulty of ſoldering the glaſs ball to the tube of the thermometer without at the ſame time either cloſing up, or otherwiſe injuring, the bore of the tube, I had recourſe to another contrivance much more commodious, and much eaſier in the execution.

At the end of a glaſs tube or cylinder about eleven inches in length, and near three quarters of an inch in diameter internally, I cauſed a hollow globe to be blown 1½ inch in diameter, with an opening in the bottom of it correſponding with the bore of the tube, and equal to it in diameter, leaving to the opening a neck or ſhort tube, about an inch in length. Having a thermometer prepared, whoſe bulb was juſt half an inch in diameter, and whoſe freezing point fell at about 2¾ inches above its bulb, I graduated its tube according to Reaumur's ſcale, beginning at 0°, and marking that point, and alſo every tenth degree above it to 80°, with threads of fine ſilk bound round it, which being moiſtened with lac varniſh adhered firmly to the tube. This thermometer I introduced into the glaſs cylinder and globe juſt deſcribed by the opening in the bottom of the globe, having firſt choaked the cylinder at about 2 inches from its junction with the globe by heating it, and crowding its ſides inwards towards its axis, leaving only an opening ſufficient to admit the tube of the thermometer. The thermometer being introduced into the cylinder in ſuch a manner that the center of its bulb coincided with the center of the globe, I marked a place in the cylinder, about three quarters of an inch above the 80th degree or boiling point upon the tube of the incloſed thermometer, and taking out the thermometer, I choaked the cylinder again in this place.

Introducing now the thermometer for the laſt time, I cloſed the opening at the bottom of the globe at the lamp, taking care before I brought it to the fire, to turn the cylinder upſide down, and to let the bulb of the thermometer fall into the cylinder till it reſted upon the lower choak in the cylinder. By this means the bulb of the thermometer was removed more than 3 inches from the flame of the lamp. The opening at the bottom of the globe being now cloſed, and the bulb of the thermometer being ſuffered to return into the globe, the end of the cylinder was cut off to within about half an inch of the upper choak. This being done, it is plain, that the tube of the thermometer projected beyond the end of the cylinder. Taking hold of the end of the tube, I placed the bulb of the thermometer as nearly as poſſible in the center of the globe, and obſerving and marking a point in the tube immediately above the upper choak of the cylinder, I turned the cylinder upſide down, and ſuffering the bulb of the thermometer to enter the cylinder, and reſt upon the firſt or lower choak, (by which means the end of the tube of the thermometer came further out of the cylinder) the end of the tube was cut off at the mark juſt mentioned, (care having firſt been taken to melt the internal cavity or bore of the tube together at that place) and a ſmall ſolid ball of glaſs, a little larger than the internal diameter or opening of the choak, was ſoldered to the end of the tube, forming a little button or knob, which reſting upon the upper choak of the cylinder ſerved to ſuſpend the thermometer in ſuch a manner that the center of its bulb coincided with the center of the globe in which it was ſhut up. The end of the cylinder above the upper choak being now heated and drawn out to a point, or rather being formed into the figure of the fruſtum of a hollow cone, the end of it was ſoldered to the end of a barometrical tube, by the help of which the cavity of the cylinder and globe containing the thermometer was completely voided of air with mercury; when, the end of the cylinder being hermetically ſealed, the barometrical tube was detached from it with a file, and the thermometer was left completely ſhut up in a Torricellian vacuum, the center of the bulb of the thermometer being confined in the center of the glaſs globe, without touching it in any part, by means of the two choaks in the cylinder, and the button upon the end of the tube. (See Fig. 2.)

Of theſe inſtruments I provided myſelf with two, as nearly as poſſible of the ſame dimenſions; the one, which I ſhall call N° 1. being voided of air, in the manner above deſcribed; the other, N° 2. being filled with air, and hermetically ſealed.

With theſe two inſtruments (ſee Fig. 2.) I made the following Experiments upon the 11th of July laſt at Manheim, between the hours of ten and twelve, the weather being very fine and clear, the mercury in the barometer ſtanding at 27 inches 11 lines, Reaumur's thermometer at 15°, and the quill hygrometer of the academy of Manheim at 47°.

Experiments, No. 3, 4, 5, and 6.

Putting both the inſtruments into a mixture of pounded ice and water, I let them remain there till the mercury in the incloſed thermometers reſted at the point 0°, that is to ſay, till they had acquired exactly the temperature of the cold mixture; and then taking them out of it I plunged them ſuddenly into a large veſſel of boiling water, and obſerved the time required for the mercury to riſe in the thermometers from ten degrees to ten degrees, from 0° to 80°, taking care to keep the water conſtantly boiling during the whole of this time, and taking care alſo to keep the inſtruments immerſed to the ſame depth, that is to ſay, juſt ſo deep that the point 0° of the incloſed thermometer was even with the ſurface of the water.

Theſe Experiments I repeated twice with the utmoſt care; and the following Table gives the reſult of them.

Thermometer N° 1.

Its bulb half an inch in diameter, ſhut up in the center of a hollow glaſs globe, $1\frac{1}{2}$ inch in diameter, *void of air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.		Heat acquired.
Exp. N° 3.	Exp. N° 4.	
		0°
M. S.	M. S.	
0 51	0 51	10
0 59	0 59	20
1 1	1 2	30
1 18	1 22	40
1 24	1 23	50
2 0	1 51	60
3 30	3 6	70
11 41	10 27	80

22 44 21 1 = total time of heating from 0° to 80°.

Total time from 0° to 70°:

M. S.

In Exp. N° 3. = 11 3

In Exp. N° 4. = 10 34

Medium = 10 $48\frac{1}{2}$

Thermometer N° 2.

Its bulb half an inch in diameter, ſhut up in the center of a hollow glaſs globe, $1\frac{1}{2}$ inch in diameter, *filled with air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.		Heat acquired.
Exp. N° 5.	Exp. N° 6.	
		0°
M. S.	M. S.	
0 30	0 30	10
0 35	0 37	20
0 41	0 41	30
0 49	0 53	40
1 1	0 59	50
1 24	1 20	60
2 45	2 25	70
9 10	9 38	80

16 55 17 3 = total time of heating from 0° to 80°.

Total time from 0° to 70°:

M. S.

In Exp. N° 5. = 7 45

In Exp. N° 6. = 7 25

Medium = 7 35

It appears from theſe Experiments that the conducting power of air to that of the Torricellian vacuum, under the circumſtances deſcribed, is as $7\frac{35}{60}$ to $10\frac{48\frac{1}{2}}{60}$ inverſely, or as 1000 to 702 nearly; for the quantities of Heat communicated being equal, the intenſity of the communication is as the times inverſely.

In theſe Experiments the Heat paſſed through the ſurrounding medium *into* the bulb of the thermometer:

meter: in order to reverſe the Experiment, and make the Heat paſs *out of* the thermometer, I put the inſtruments into boiling water, and let them remain therein till they had acquired the temperature of the water; that is to ſay, till the mercury in the incloſed thermometers ſtood at 80°; and then, taking them out of the boiling water, I plunged them ſuddenly into a mixture of water and pounded ice, and moving them about continually in this mixture, I obſerved the times employed in cooling as follows:

Thermometer N° 1. Surrounded by a *Torricellian vacuum.* *Taken out of boiling water, and plunged into freezing water.*			*Thermometer* N° 2. Surrounded by *air.* *Taken out of boiling water, and plunged into freezing water.*		
Time elapſed.		Heat loſt.	Time elapſed.		Heat loſt.
Exp. N° 7.	Exp. N° 8.		Exp. N° 9.	Exp. N° 10.	
		80°			80°
M. S.	M. S.	°	M. S.	M. S.	°
1 2	0 54	70	0 33	0 33	70
0 58	1 2	60	0 39	0 34	60
1 17	1 18	50	0 44	0 44	50
1 46	1 37	40	0 55	0 55	40
2 5	2 16	30	1 17	1 18	30
3 14	3 10	20	1 57	1 57	20
5 42	5 59	10	3 44	3 40	10
Not obſerved.	Not obſerved.	0	40 10	Not obſerved.	0
Total time of cooling from 80° to 10°.			Total time of cooling from 80° to 10°.		
		M. S.			M. S.
In Exp. N° 7.		=16 4	In Exp. N° 9.		=9 49
In Exp. N° 8.		=16 16	In Exp. N° 10.		=9 41
	Medium	=16 10		Medium	=9 45

By theſe Experiments it appears, that the conducting power of air is to that of the Torricellian vacuum as $9\frac{45}{60}$ to $16\frac{10}{60}$ inverſely, or as 1000 to 603.

To determine whether the ſame law would hold good when the heated thermometers, inſtead of being plunged into freezing water, were ſuffered to cool in the open air, I made the following Experiments. The thermometers N° 1. and N° 2. being again heated in boiling water, as in the laſt Experiments, I took them out of the water, and ſuſpended them in the middle of a large room, where the air (which appeared to be perfectly at reſt; the windows and doors being all ſhut) was warm to the 16th degree of REAUMUR's thermometer, and the times of cooling were obſerved as follows:

(Exp. N° 11.) *Thermometer* N° 1. Surrounded by a Torricellian vacuum, *Heated to* 80°, *and ſuſpended in the open air warm to* 16°.		(Exp. N° 12.) *Thermometer* N° 2. Surrounded by air. *Heated to* 80°, *and ſuſpended in the open air warm to* 16°.	
Time elapſed.	Heat loſt.	Time elapſed.	Heat loſt.
——	80°	——	80°
M. S.	°	M. S.	°
Not obſerved.	70	Not obſerved.	70
1 24	60	0 51	60
1 44	50	1 5	50
2 28	40	1 34	40
4 16	30	2 41	30
10 12 = total time employed in cooling from 70° to 30°.		6 11 = total time employed in cooling from 70° to 30°.	

Here the difference in the conducting powers of air and of the Torricellian vacuum appears to be nearly the ſame as in the foregoing Experiments, being as $6\frac{11}{60}$ to $10\frac{13}{60}$ inverſely, or as 1000 to 605. I could not obſerve the time of cooling from 80° to 70°, being at that time buſied in ſuſpending the inſtruments.

As it might poſſibly be objected to the concluſions drawn from theſe Experiments that, notwithſtanding all the care that was taken in the conſtructing of the two inſtruments made uſe of that they ſhould be perfectly alike, yet they might in reality be ſo far different either in ſhape or ſize, as to occaſion a very ſenſible error in the reſult of the Experiments; to remove theſe doubts I made the following Experiments:

In the morning towards eleven o'clock, the weather being remarkably fine, the mercury in the barometer ſtanding at 27 inches 11 lines, Reaumur's thermometer at 15°, and the hygrometer at 47°, I repeated the Experiment N° 3. (of heating the thermometer N° 1. in boiling water, &c.) and immediately afterwards opened the cylinder containing the thermometer at its upper end, where it had been ſealed, and letting the air into it, I re-ſealed it hermetically, and repeated the Experiment again with the ſame inſtrument, the thermometer being now ſurrounded with air, like the thermometer N° 2.

The reſult of theſe Experiments, which may be ſeen in the following Table, ſhews evidently, that the error ariſing from the difference of the ſhapes or dimenſions of the two inſtruments in queſtion was inconſiderable, if not totally imperceptible.

(Exp. N° 13.) *Thermometer* N° 1. Its bulb half an inch in diameter, ſhut up in the center of a glaſs globe, 1½ inch in diameter, *voided of air*, and hermetically ſealed. *Taken out of freezing water, and plunged into boiling water.*		(Exp. N° 14.) *The ſame Thermometer* (N° 1.) The glaſs globe, containing the bulb of the thermometer, being now *filled with air*, and hermetically ſealed. *Taken out of freezing water, and plunged into boiling water.*	
Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
——	0°	——	0°
M. S.	0	M. S.	0
0 55	10	0 32	10
0 55	20	0 32	20
1 7	30	0 43	30
1 15	40	0 50	40
1 29	50	1 1	50
2 2	60	1 24	60
3 21	70	2 38	70
13 44	80	10 25	80
24 48 = total time of heating from 0° to 80°. Total time from 0° to 70° = 11′ 4″.		18 9 = total time of heating from 0° to 80°. Total time from 0° to 70° = 7′ 40″.	

It appears, therefore, from theſe Experiments, that the conducting power of common atmoſpheric air is to that of the Torricellian vacuum as $7\frac{40}{60}$ to $11\frac{4}{60}$ inverſely, or as 1000 to 602; which differs but very little from the reſult of all the foregoing Experiments.

Notwithſtanding that it appeared, from the reſult of theſe laſt Experiments, that any difference there might poſſibly have been in the forms or dimenſions of the inſtruments N° 1. and N° 2. could hardly have produced any ſenſible error in the reſult of the Experiments in queſtion; I was willing, however, to ſee how far any conſiderable alterations of ſize in the inſtrument would affect the Experiment: I therefore provided myſelf with another inſtrument which I ſhall call *Thermometer* N° 3. different from thoſe already deſcribed in ſize, and a little different in its conſtruction.

The bulb of the thermometer was of the ſame form and ſize as in the inſtruments N° 1. and N° 2. that is to ſay, it was globular, and half an inch in diameter; but the glaſs globe, in the center of which it was confined, was much larger, being 3 inches 7½ lines in diameter; and the bore of the tube of the thermometer was much finer, and conſequently its length, and the diviſions of its ſcale, were greater. The diviſions were marked upon the tube with threads of ſilk of different colours at every tenth degree, from 0° to 80°, as in the before-mentioned inſtruments. The tube or cylinder belonging to the glaſs globe was 8 lines in diameter, a little longer than the tube of the thermometer, and perfectly cylindrical from its upper end to its junction with the globe, being without any choak; the thermometer being confined in the center of the globe by a different contrivance, which was as follows. To the opening of the cylinder was fitted a ſtopple of dry wood, covered with a coating of hard varniſh, through the center or axis of which paſſed the end of the tube of the thermometer: this ſtopple confined the tube in the axis of the cylinder at its upper end. To confine it at its lower end, there was fitted to it a ſmall ſteel ſpring, a little below the point 0°; which, being faſtened to the tube of the thermometer, had three elaſtic points projecting outwards, which preſſing againſt the inſide of the cylinder, confined the thermometer in its place. The total length of this inſtrument, from the bottom of the globe to the upper end of the cylinder, was 18 inches, and the freezing point upon the thermometer fell about 3 inches above the bulb; conſequently this point lay about 1½ inch above the junction of the cylinder with the globe, when the thermometer was confined in its place, the center of its bulb coinciding with the center of the globe. Through the ſtopple which cloſed the end of the cylinder, paſſed two ſmall glaſs tubes, about a line in diameter, which being about a line longer than the ſtopple were cloſed occaſionally with ſmall ſtopples fitted to their bores. Theſe tubes (which were fitted exactly in the holes bored in the great ſtopple of the cylinder to receive them, and fixed in their places with cement) ſerved to convey air, or any other fluid, into the glaſs ball, without its being neceſſary to remove the ſtopple cloſing the end of the cylinder; which ſtopple, in order to prevent the poſition of the thermometer from being eaſily deranged, was cemented in its place.

I have been the more particular in the deſcription of theſe inſtruments, as I conceive it to be abſolutely neceſſary to have a perfect idea of them in order to judge of the Experiments made with them, and of their reſults.

With the inſtrument laſt deſcribed (which I have called *Thermometer* N° 3.) I made the following

Experiment. It was upon the 18th of July 1785, in the afternoon, the weather variable, alternate clouds and ſun-ſhine; wind ſtrong at S. E. with now and then a ſprinkling of rain; barometer at 27 inches 10¾ lines, thermometer at 18°¼, and hygrometer variable from 44° to extreme moiſture.

In order to compare the reſult of the Experiment made with this inſtrument with thoſe made with the thermometer Nº 2. I have placed together in the ſame Table the different Experiments made with them.

(Exp. Nº 15.) *Thermometer* Nº 3. Its bulb half an inch in diameter, ſhut up in the center of a glaſs globe, 3 inches 7½ lines in diameter, and ſurrounded by air. *Taken out of freeſing water, and plunged into boiling water.*		(Exp. Nº 4 and Nº 5.) *Thermometer* Nº 2. Its bulb half an inch in diameter, ſhut up in the center of a glaſs globe, 1½ inch in diameter, and ſurrounded by air. *Taken out of freeſing water, and plunged into boiling water.*			
		Time elapſed.			Heat acquired.
Time elapſed.	Heat acquired	Exp. Nº 4.	Exp. Nº 5.	Medium.	
	0°				0°
M. S.	0	M. S.	M. S.	M. S.	0
0 33	10	0 30	0 30	0 30	10
0 38	20	0 35	0 37	0 36	20
0 54	30	0 41	0 41	0 41	30
0 51	40	0 49	0 53	0 51	40
1 7	50	1 1	0 59	1 0	50
1 28	60	1 24	1 20	1 22	60
2 28	70	2 45	2 25	2 35	70
9 0	80	9 10	9 38	9 24	80
16 59 = total time of heating from 0° to 80°.		16 55	17 3	16 59 = total time of heating from 0° to 80°.	
Time from 0° to 70° = 7′ 59″.		Time from 0° to 70° = 7′ 35″.			

If the agreement of theſe Experiments with the thermometers Nº 2. and Nº 3. ſurpriſed me, I was not leſs ſurpriſed with their diſagreement in the Experiment which follows:

Experi-

Experiment, N° 16.

Taking the thermometer N° 3. out of the boiling water, I immediately ſuſpended it in the middle of a large room, where the air, which was quiet, was at the temperature of 18°¼ R. and obſerved the times of cooling as follows:

Time elapſed.	Heat loſt.
——	80°
M. S.	°
1 55	70
0 12	60
1 33	50
2 15	40
4 0	30
——	

9 55 = total time of cooling from 80° to 30°.

Time from 70° to 30° = 8′ 0″; but in the Experiment N° 12. with the thermometer N° 2. the time employed in cooling from 70° to 30° was only 6′ 11″. In this Experiment, with the thermometer N° 3. the time employed in cooling from 60° to 30° was 7′ 48″; but in the above-mentioned Experiment, with the thermometer N° 2. it was only 5′ 20″. It is true, the air of the room was ſomewhat cooler when the former Experiment was made, than when this latter was made, with the thermometer N° 3.; but this difference of temperature, which was only 2°¼, (in the former caſe the thermometer in the room ſtanding at 16°, and in the latter at 18°¼,) certainly could not have occaſioned the whole of the apparent difference in the reſults of the Experiments.

Does air receive Heat more readily than it parts with it? This is a queſtion highly deſerving of further inveſtigation, and I hope to be able to give it a full examination in the courſe of my projected inquiries; but leaving it for the preſent, I ſhall proceed to give an account of the Experiments which I have already made.

Conceiving it to be a ſtep of conſiderable importance towards coming at a further knowledge of the nature of Heat, to aſcertain, by indiſputable evidence, its paſſage through the Torricellian vacuum, and to determine, with as much preciſion as poſſible, the law of its motions in that medium; and being apprehenſive that doubts might ariſe with reſpect to the Experiments before deſcribed, on account of the contact of the tubes of the incloſed thermometers in the inſtruments made uſe of with the containing glaſs globes, or rather with their cylinders: by means of which (it might be ſuſpected) that a certain quantity, if not all the Heat acquired, might poſſibly be communicated; to put this matter beyond all doubt, I made the following Experiment.

In the middle of a glaſs body, of a pear-like form, about 8 inches long, and 2½ inches in its greateſt diameter, I ſuſpended a ſmall mercurial thermometer, 5½ inches long, by a fine thread of ſilk, in ſuch a manner that neither the bulb of the thermometer, nor its tube, touched the containing glaſs body in any part. The tube of the thermometer was graduated, and marked with fine threads of ſilk of different colours, bound round it, as in the thermometers belonging to the other inſtruments already deſcribed; and the thermometer was ſuſpended in its place by means of a ſmall ſteel ſpring, to which the end of the thread of ſilk which held the thermometer being attached, it (the ſpring) was forced into a ſmall globular protuberance or cavity, blown in the upper extremity of the glaſs body, about half an inch in diameter, where the ſpring remaining, the thermometer neceſſarily remained ſuſpended in the axis of the glaſs body. There was an opening at the bottom of the glaſs body, through which the thermometer was introduced; and a barometrical tube being ſoldered to this opening, the inſide of the glaſs body was voided of air by means of mercury; and this opening being afterwards ſealed hermetically, and the barometrical tube being taken away, the thermometer was left ſuſpended in a Torricellian vacuum.

In this inſtrument, as the incloſed thermometer did not touch the containing glaſs body in any part, on the contrary, being diſtant from its internal ſurface an inch or more in every part, it is clear, that whatever Heat paſſed *into* or *out of* the thermometer muſt have paſſed *through* the ſurrounding Torricellian vacuum: for it cannot be ſuppoſed, that the fine thread of ſilk, by which the thermometer was ſuſpended, was capable of conducting any Heat at all, or at leaſt any ſenſible quantity. I therefore flattered myſelf with hopes of being able, with the aſſiſtance of this inſtrument, to determine poſitively with regard to the paſſage of Heat in the Torricellian vacuum: and this, I think, I have done, notwithſtanding an unfortunate accident that put it out of my power to purſue the Experiment ſo far as I intended.

This inſtrument being fitted to a ſmall ſtand or foot of wood, in ſuch a manner that the glaſs body remained in a perpendicular ſituation, I placed it in my room, by the ſide of another incloſed thermometer (N° 2.), which was ſurrounded by air, and obſerved the effects produced on it by the variation of Heat in the atmoſphere. I ſoon diſcovered, by the motion of the mercury in the incloſed thermometer, that the Heat paſſed through the Torricellian vacuum; but it appeared plainly from the ſluggiſhneſs, or great inſenſibility of the thermometer, that the Heat paſſed with much greater difficulty in this medium than in common air. I now plunged both the thermometers into a bucket of cold water; and I obſerved that the mercury in the thermometer ſurrounded by air deſcended much faſter than that in the thermometer ſurrounded by the Torricellian vacuum. I took them out of the cold water, and plunged them into a veſſel of hot water (having no conveniencies at hand to repeat the Experiment in due form with the freezing and with the boiling water); and the thermometer ſurrounded by the Torricellian vacuum appeared ſtill to be much more inſenſible or ſluggiſh than that ſurrounded by air.

Theſe trials were quite ſufficient to convince me of the paſſage of Heat in the Torricellian vacuum, and alſo of the greater difficulty of its paſſage in that medium than in common air; but, not ſatisfied to reſt my inquiries here, I took the firſt opportunity that offered, and ſet myſelf to repeat the Experiments which I had before made with the inſtruments N° 1 and N° 2. I plunged this inſtrument into a mixture of pounded ice and water, where I let it remain till the mercury in the incloſed thermometer had deſcended to 0°; when, taking out of this cold mixture, I plunged it ſuddenly into a veſſel of boiling water, and prepared myſelf to obſerve the aſcent of the mercury in the incloſed thermometer, as in the foregoing Experiments; but unfortunately the moment the end of the glaſs body touched the boiling water, it cracked with the Heat at the point where it had been hermetically ſealed, and the water ruſhing into the body, ſpoiled the Experiment; and I have not ſince had an opportunity of providing myſelf with another inſtrument to repeat it.

It having been my intention from the beginning to examine the conducting powers of the artificial airs or gaſſes, the thermometer N° 3. was conſtructed with a view to thoſe Experiments; and having now provided myſelf with a ſtock of thoſe different kinds of airs, I began with *fixed air*, with which, by means of water, I filled the globe and cylinder containing the thermometer; and ſtopping up the two holes in the great ſtopple cloſing the end of the cylinder, I expoſed the inſtrument in freezing water till the mercury in the incloſed thermometer had deſcended to 0°; when, taking it out of the freezing water, I plunged it into a large veſſel of boiling water, and prepared myſelf to obſerve the times of heating, as in the former caſes; but an accident happened, which ſuddenly put a ſtop to the Experiment. Immediately upon plunging the inſtrument into the boiling water, the mercury began to riſe in the thermometer with ſuch uncommon celerity, that it had paſſed the firſt diviſion upon the tube (which marked the 10th degree, according to Reaumur's ſcale) before I was aware of its being yet in motion; and having thus miſſed the opportunity of obſerving the time elapſed when the mercury arrived at that point, I was preparing to obſerve its paſſage of the next, when all of a ſudden the ſtopple cloſing the end of the cylinder was blown up the chimney with a great exploſion, and the thermometer, which, being cemented to it by its tube, was taken along with it, and was broken to pieces, and deſtroyed in its fall.

This unfortunate Experiment, though it put a ſtop for the time to the inquiries propoſed, opened the way to other reſearches not leſs intereſting. Suſpecting that the exploſion was occaſioned by the rarefaction of the water which remained attached to the inſide of the globe and cylinder after the operation of filling them with fixed air; and thinking it more than probable, that the uncommon celerity with which the mercury roſe in the thermometer was principally owing to the ſame cauſe, I was led to examine the conducting power of *moiſt air*, or air ſaturated with water.

For this Experiment I provided myſelf with a new thermometer N° 4. the bulb of which, being of the ſame form as thoſe already deſcribed (*viz.* globular) was alſo of the ſame ſize, or half an inch in diameter. To receive this thermometer a glaſs cylinder was provided, 8 lines in diameter, and about 14 inches long, and terminated at one end by a globe 1½ inch in diameter. In the center of this globe the bulb of the thermometer was confined, by means of the ſtopple which cloſed the end of the cylinder; which ſtopple, being near 2 inches long, received the end of the tube of the thermometer into a hole bored through its center or axis, and confined the thermometer in its place, without the aſſiſtance of any other apparatus. Through this ſtopple two other ſmall holes were bored, and lined with thin glaſs tubes, as in the thermometer N° 3. opening a paſſage into the cylinder, which holes were occaſionally ſtopped up with ſtopples of cork; but to prevent accidents, ſuch as I have before experienced from an exploſion, great care was taken not to preſs theſe ſtopples into their places with any conſiderable force, that they might the more eaſily be blown out by any conſiderable effort of the confined air, or vapour.

Though in this inſtrument the thermometer was not altogether ſo ſteady in its place as in the thermometers N° 1, N° 2, and N° 3. the elaſticity of the tube, and the weight of the mercury in the bulb of the thermometer, occaſioning a ſmall vibration

(Exp. N° 19.) *Thermometer* N° 4. Surrounded by *moiſt air.* *Taken out of boiling water, and plunged into freezing water.*		(Exp. N° 10.) *Thermometer* N° 2. Surrounded by *dry air.* *Taken out of boiling water, and plunged into freezing water.*	
Time elapſed.	Heat loſt.	Time elapſed.	Heat loſt.
	80°		80°
M. S.	0	M. S.	0
0 4	70	0 33	70
0 14	60	0 34	60
0 31	50	0 44	50
0 52	40	0 55	40
1 22	30	1 18	30
2 3	20	1 57	20
4 2	10	3 40	10
9 8 = total time of cooling from 80° to 10°.		9 12 = total time of cooling from 80° to 10°.	

Though the difference of the whole times of cooling from 80° to 10° in theſe two Experiments appears to have been very ſmall, yet the difference of the times taken up by the firſt twenty or thirty degrees from the boiling point is very remarkable, and ſhows with how much greater facility Heat paſſes in moiſt air than in dry air. Even the ſlowneſs with which the mercury in the thermometer N° 4. deſcended in this Experiment from the 30th to the 20th, and from the 20th to the 10th degree, I attribute in ſome meaſure to the great conducting power of the moiſt air with which it was ſurrounded; for the cylinder containing the thermometer and the moiſt air, being not wholly ſubmerged in the freezing water, that part of it which remained out of the water was neceſſarily ſurrounded by the air of the atmoſphere; which being much warmer than the water, communicated a part of its Heat to the glaſs; which, paſſing from thence into the contained moiſt air as ſoon as that air became colder than the external air, was, through that medium, communicated to the bulb of the incloſed thermometer, which prevented its cooling ſo faſt as it would otherwiſe have done. But when the weather becomes cold, I propoſe to repeat this Experiment with variations, in ſuch a manner as to put the matter beyond all doubt. In the mean time I cannot help obſerving, with what infinite wiſdom and goodneſs Divine Providence appears to have guarded us againſt the evil effects of exceſſive Heat and Cold in the atmoſphere; for if it were poſſible for the air to be equally damp during the ſevere cold of the winter months as it ſometimes is in ſummer, its conducting power, and conſequently its apparent coldneſs, when applied to our bodies, would be ſo much increaſed, by ſuch an additional degree of moiſture, that it would become quite intolerable; but, happily for us, its power to hold water in ſolution is diminiſhed, and with that its power to rob us of our animal heat, in proportion as its coldneſs is increaſed.

Every body knows how very diſagreeable a moderate degree of cold is when the air is very damp; and from hence it appears, why the thermometer is not always a juſt meaſure of the apparent or ſenſible Heat of the atmoſphere.

If colds or catarrhs are occaſioned by our bodies being robbed of our animal heat, the reaſon is plain why thoſe diſorders prevail moſt during the cold autumnal rains, and upon the breaking up of the froſt in the ſpring. It is likewiſe plain from whence it is that ſleeping in damp beds, and inhabiting damp houſes is ſo very dangerous; and why the evening air is ſo pernicious in ſummer and in autumn, and why it is not ſo during the hard froſts of winter.

It has puzzled many very able philoſophers and phyſicians to account for the manner in which the extraordinary degree or rather *quantity* of Heat is generated which an animal body is ſuppoſed to loſe, when expoſed to the cold of winter, above what it communicates to the ſurrounding atmoſphere in warm ſummer weather; but is it not more than probable, that the difference of the quantities of Heat, actually loſt or communicated, is infinitely leſs than what they have imagined?

Theſe inquiries are certainly very intereſting; and they are undoubtedly within the reach of well contrived and well conducted Experiments. But taking my leave for the preſent of this curious ſubject of inveſtigation, I haſten to the ſequel of my Experiments.

Finding ſo great a difference in the conducting powers of common air and of the Torricellian vacuum, I was led to examine the conducting powers of common air of different degrees of denſity. For this Experiment I prepared the thermometer N° 4. by ſtopping up one of the ſmall glaſs tubes paſſing through the ſtopple, and opening a paſſage into the cylinder, and by fitting a valve to the external aperture of the other. The inſtrument, thus prepared, being put under the receiver of an air-pump, the air paſſed freely out of the globe and cylinder upon working the machine, but the valve above deſcribed prevented its return upon letting air into the receiver. The gage of the air-pump ſhowed the degree of rarity of the air under the receiver, and conſequently of that filling the globe and cylinder, and immediately ſurrounding the thermometer.

With this inſtrument, the weather being clear and fine, the mercury in the barometer ſtanding at 27 inches 9 lines, the thermometer at 15°, and the hygrometer at 47°, I made the following Experiments.

(Exp. N° 20.) *Thermometer* N° 4. Surrounded by common air, barometer ſtanding at 27 inches 9 lines. *Taken out of freezing water, and plunged into boiling water.*		(Exp. N° 21.) *Thermometer* N° 4. Surrounded by air rarefied by pumping till the barometer-gage ſtood at 6 inches 11½ lines. *Taken out of freezing water, and plunged into boiling water.*		(Exp. N° 22.) *Thermometer* N° 4. Surrounded by air rarefied by pumping till the barometer-gage ſtood at 1 inch 2 lines. *Taken out of freezing water, and plunged into boiling water.*	
Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
——	0°	——	0°	——	0°
M. S.	0	M. S.	0	M. S.	0
0 31	10	0 31	10	0 29	10
0 40	20	0 38	20	0 36	20
0 41	30	0 44	30	0 49	30
0 47	40	0 51	40	1 1	40
1 4	50	1 7	50	1 1	50
1 25	60	1 19	60	1 24	60
2 28	70	2 27	70	2 31	70
10 17	80	10 21	80	not obſerved.	80
7 36 = total time of heating from 0° to 70°.		7 37 = total time of heating from 0° to 70°.		7 51 = total time of heating from 0° to 70°.	

The reſult of theſe Experiments, I confeſs, ſurpriſed me not a little; but the diſcovery of truth being the ſole object of my inquiries (having no favourite theory to defend) it brings no diſappointment along with it, under whatever unexpected ſhape it may appear. I hope that further Experiments may lead to the diſcovery of the cauſe why there is ſo little difference in the conducting powers of air of ſuch very different degrees of rarity, while there is ſo great a difference in the conducting powers of air, and of the Torricellian vacuum. At preſent, I ſhall not venture any conjectures upon the ſubject; but in the mean time I dare to aſſert, that the Experiments I have made may be depended on.

The time of my ſtay at Manheim being expired (having had the honour to attend thither his moſt Serene Highneſs the Elector Palatine, reigning Duke of Bavaria, in his late journey), I was prevented from purſuing theſe inquiries further at that time; but I ſhall not fail to recommence them the firſt leiſure moment I can find, which I fancy will be about the beginning of the month of November. In the mean time, to enable myſelf to purſue them with effect, I am ſparing neither labour nor expence in providing a complete apparatus neceſſary for my purpoſe; and his Electoral Highneſs has been graciouſly pleaſed to order M. Artaria (who is in his ſervice) to come to Munich to aſſiſt me. With ſuch a Patron as his moſt Serene Highneſs, and with ſuch an aſſiſtant as Artaria, I ſhall go on in my purſuits with chearfulneſs. Would to God that my labours might be as uſeful to others as they will be pleaſant to me!

I ſhall conclude this chapter with a ſhort account of ſome Experiments I have made to determine the conducting powers of water and of mercury; and with a table, ſhowing at one view the conducting powers of all the different mediums which I have examined.

Having filled the glaſs globe incloſing the bulb of the thermometer N° 4, firſt with water, and then with mercury, I made the following Experiments, to aſcertain the conducting powers of thoſe two Fluids.

(Exp. N° 23.) *Thermometer* N° 4. Surrounded by *water.* *Taken out of freezing water, and plunged into boiling water.*		(Exp. N° 24, 25, and 26.) *Thermometer* N° 4. Surrounded by *mercury.* *Taken out of freezing water, and plunged into boiling water.*			
Time elapſed.	Heat acquired.	Time elapſed. Ex. N° 24.	Ex. N° 25.	Ex. N° 26.	Heat acquired.
	0°				0°
M. S.	0	M. S.	M. S.	M. S.	
0 19	10	0 5	0 5	0 5	10
0 8	20	0 4	0 2	0 5	20
0 9	30	0 2	0 2	0 4	30
0 11	40	0 4	0 5	0 5	40
0 15	50	0 4	0 4	0 7	50
0 21	60	0 7	0 4	0 8	60
0 34	70	0 15	0 9	0 14	70
2 13	80	Not obſerved.	0 58	Not obſerved.	80
1 57=total time of heating from 0° to 70°.		0 41	0 31	0 48 = total times of heating from 0° to 70°.	

The total times of heating from 0° to 70° in the three Experiments with mercury being 41 ſeconds, 31 ſe-

31 ſeconds, and 48 ſeconds, the mean of theſe times is $36\frac{1}{3}$ ſeconds; and as in the Experiment with water the time employed in acquiring the ſame degree of Heat was 1′ 57″ = 117 ſeconds, it appears from theſe Experiments, that the conducting power of mercury to that of water, under the circumſtances deſcribed, is as $36\frac{1}{3}$ to 117 inverſely, or as 1000 to 313. And hence it is plain, why mercury *appears* ſo much hotter, and ſo much colder, to the touch than water, when in fact it is of the ſame temperature: for the force or violence of the ſenſation of what appears *hot* or *cold* depends not entirely upon the temperature of the body exciting in us thoſe ſenſations, or upon the degree of Heat it actually poſſeſſes, but upon the *quantity* of Heat it is capable of communicating to us, or receiving from us, in any given ſhort period of time, or it is as the *intenſity of the communication*; and this depends in a great meaſure upon the conducting powers of the bodies in queſtion.

The ſenſation excited in us when we touch any thing that appears to us to be *hot* is the entrance of Heat into our bodies; that of *cold* is its exit; and whatever contributes to facilitate or accelerate this communication adds to the violence of the ſenſation. And this is another proof that the thermometer cannot be a juſt meaſure of the intenſity of the *ſenſible* Heat, or Cold, exiſting in bodies; or rather, that the touch does not afford us a juſt indication of their *real* temperatures.

A TABLE

A TABLE *of the* CONDUCTING POWERS *of the under-mentioned* MEDIUMS *as determined by the foregoing Experiments.*

Therm. Nº 1.	Thermometer Nº 4.							
Taken out of freezing water; and plunged into boiling water.								
Time elapſed.								
Torricellian Vacuum (Exp. Nº 3, 4. and 13.)	Common air, denſity=1, (Exp. Nº 20.)	Rarefied air, denſity=¼ (Exp. Nº 21.)	Rarefied air, denſity=$\frac{1}{24}$ (Exp. Nº 22.)	Moiſt air (Exp. Nº 18.)	Water (Exp. Nº 23.)	Mercury (Exp. Nº 24, 25, and 26.)	Heat acquired.	
M. S.	M. S.	M. S.	M. S.	M. S.	M S.	M. S.	0°	
							0	
0 52	0 31	0 31	0 29	0 6	0 19	0 5	10	
0 58	0 40	0 38	0 36	0 4	0 8	0 3⅔	20	
1 3	0 41	0 44	0 49	0 5	0 9	0 2⅔	30	
1 18	0 47	0 51	1 1	0 9	0 11	0 4⅔	40	
1 25	1 4	1 7	1 1	0 18	0 15	0 5	50	
1 58	1 25	1 19	1 24	0 26	0 21	0 6⅓	60	
3 19	2 28	2 27	2 31	0 43	0 34	0 12⅔	70	
11 57	10 17	10 21	——	7 45	2 13	0 58	80	
10 53	7 36	7 37	7 51	1 51	1 57	0 36⅔	=total	
times of heating from 0° to 70°.								

In determining the relative conducting powers of theſe mediums, I have compared the times of the heating of the thermometers from 0° to 70° inſtead of taking the whole times from 0° to 80°, and this I have done on account of the ſmall variation in the Heat of the boiling water ariſing from the variation of the weight of the atmoſphere, and alſo on account of the very ſlow motion of the mercury between the 70th and the 80th degrees, and the difficulty of determining the preciſe moment when the mercury arrives at the 80th degree.

Taking now the conducting power of mercury =1000, the conducting powers of the other mediums, as determined by theſe Experiments, will be as follows, *viz.*

Mercury	1000
Moiſt air	330
Water	313
Common air, denſity=1	$80\frac{41}{100}$
Rarefied air, denſity=$\frac{1}{4}$	$80\frac{23}{100}$
Rarefied air, denſity=$\frac{1}{24}$	78
The Torricellian vacuum	55

And in theſe proportions are the quantities of Heat which theſe different mediums are capable of tranſmitting in any given time; and conſequently theſe numbers expreſs the relative *ſenſible* temperatures of the mediums, as well as their conducting powers. How far theſe deciſions will hold good under a variation of circumſtances experiment only can determine. This is certainly a ſubject of inveſtigation not leſs curious in itſelf than it is intereſting to mankind; and I wiſh that what I have done may induce others to turn their attention to this long neglected field of experimental inquiry. For my own part, I am determined not to quit it.

In the future proſecution of theſe inquiries, I do not mean to confine myſelf ſolely to the determining

ing of the conducting powers of Fluids; on the contrary, ſolids, and particularly ſuch bodies as are made uſe of for cloathing, will be principal ſubjects of my future Experiments. I have indeed already begun theſe reſearches, and have made ſome progreſs in them; but I forbear to anticipate a matter which will be the ſubject of a future communication.

CHAP. II.

The relative Warmth of various Subſtances uſed in making artificial Cloathing, determined by Experiment.—Relative Warmth of Coverings of the ſame Thickneſs, and formed of the ſame Subſtance, but of different Denſities.—Relative Warmth of Coverings formed of equal Quantities of the ſame Subſtance, diſpoſed in different Ways.—Experiments made with a View to determine how far the Power which certain Bodies poſſeſs of confining Heat depends on their chemical Properties.—Experiments with Charcoal—*with* Lampblack—*with* Wood-aſhes—*Striking Experiments with* Semen Lycopodii.—*All theſe Experiments indicate that the Air which occupies the Interſtices of Subſtances uſed in forming Coverings for confining Heat, acts a very important Part in that Operation.—Thoſe Subſtances appear to prevent the air from conducting the Heat.—An Inquiry concerning the Manner in which this is effected.—This Inquiry leads to a deciſive Experiment from the Reſult of which it appears that* Air is a perfect Non-conductor of Heat.—*This Diſcovery affords the Means of explaining a Variety of intereſting Phenomena in the Œconomy of Nature.*

[Read before the ROYAL SOCIETY, January 19, 1792.]

THE confining and directing of Heat are objects of ſuch vaſt importance in the œconomy of human life, that I have been induced to confine my reſearches chiefly to thoſe points, conceiving that very great advantages to mankind could not fail to be derived from the diſcovery of any new facts relative to theſe operations.

If the laws of the communication of Heat from one body to another were known, meaſures might be taken with certainty, in all caſes, for confining it, and directing its operations, and this would not only be productive of great œconomy in the articles of fuel and clothing, but would likewiſe greatly increaſe the comforts and conveniencies of life; objects of which the philoſopher ſhould never loſe ſight.

The route which I have followed in this inquiry is that which I thought bid faireſt to lead to uſeful diſcoveries. Without embarraſſing myſelf with any particular theory, I have formed to myſelf a plan of experimental inveſtigation, which I conceived would conduct me to the knowledge of *certain facts*, of which we are now ignorant, or very imperfectly informed, and with which it is of conſequence that we ſhould be made acquainted.

The firſt great object which I had in view in this inquiry was to aſcertain, if poſſible, the cauſe of the warmth of certain bodies; or the circumſtances upon which their power of confining Heat depends. This, in other words, is no other than to determine the cauſe of the conducting and non-conducting power of bodies, with regard to Heat.

To this end I began by determining by actual experiment the relative conducting powers of various bodies of very different natures, both fluids and ſolids, of ſome of which Experiments I have already given an account in the Paper above mentioned, which is publiſhed in the Tranſactions of the Royal Society for the year 1786; I ſhall now, taking up the matter where I left it, give the continuation of the hiſtory of my reſearches.

Having diſcovered that the Torricellian vacuum is a much worſe conductor of Heat than common air, and having aſcertained the relative conducting powers of air, of water, and of mercury, under different circumſtances, I proceeded to examine the conducting powers of various *ſolid bodies*, and particularly of ſuch ſubſtances as are commonly made uſe of for clothing.

The method of making theſe Experiments was as follows: a mercurial thermometer, (ſee Fig. 4.) whoſe bulb was about $\frac{55}{100}$ of an inch in diameter, and its tube, about 10 inches in length, was ſuſpended in the axis of a cylindrical glaſs tube, about $\frac{3}{4}$ of an inch in diameter, ending with a globe $1\frac{6}{10}$ inch in diameter, in ſuch a manner that the centre of the bulb of the thermometer occupied the centre of the globe; and the ſpace between the internal ſurface of the globe and the ſurface of the bulb of the thermometer being filled with the ſubſtance the conducting power of which was to be determined, the inſtrument was heated in boiling water, and afterwards being plunged into a freezing mixture of pounded ice and water, the times of cooling were obſerved, and noted down.

The tube of the thermometer was divided at every tenth degree from 0°, or the point of freezing, to 80°, that of boiling water, and theſe diviſions being marked upon the tube with the point of a diamond, and the cylindrical tube being left empty, the height of the mercury in the tube of the thermometer was ſeen through it.

The thermometer was confined in its place by means of a ſtopple of cork, about 1½ inch long, fitted to the mouth of the cylindrical tube, through the centre of which ſtopple the end of the tube of the thermometer paſſed, and in which it was cemented.

The operation of introducing into the globe the ſubſtances the conducting powers of which are to be determined, is performed in the following manner: the thermometer being taken out of the cylindrical tube, about two-thirds of the ſubſtance which is to be the ſubject of the Experiment are introduced into the globe; after which, the bulb of the thermometer is introduced a few inches into the cylinder; and, after it, the remainder of the ſubſtance being placed round about the tube of the thermometer; and laſtly, the thermometer being introduced farther into the tube, and being brought into its proper place, that part of the ſubſtance which, being introduced laſt, remains in the cylindrical tube above the bulb of the thermometer, is puſhed down into the globe, and placed equally round the bulb of the thermometer by means of a braſs wire which is paſſed through holes made for

that

that purpoſe in the ſtopple cloſing the end of the cylindrical tube.

As this inſtrument is calculated merely for meaſuring the paſſage of Heat in the ſubſtance the conducting power of which is examined, I ſhall give it the name of *paſſage-thermometer*, and I ſhall apply the ſame appellation to all other inſtruments conſtructed upon the ſame principles, and for the ſame uſe, which I may in future have occaſion to mention; and as this inſtrument has been ſo particularly deſcribed, both here, and in my former Paper upon the ſubject of Heat, in ſpeaking of any others of the ſame kind in future it will not be neceſſary to enter into ſuch minute details. I ſhall, therefore, only mention their *ſizes*, or the diameters of their bulbs, the diameters of their globes, the diameters of their cylinders, and the lengths and diviſions of their tubes, taking it for granted that this will be quite ſufficient to give a clear idea of the inſtrument.

In moſt of my former Experiments, in order to aſcertain the conducting power of any body, the body being introduced into the globe of the paſſage-thermometer, the inſtrument was cooled to the temperature of freezing water, after which, being taken out of the ice water, it was plunged ſuddenly into boiling water, and the times of heating from ten to ten degrees were obſerved and noted; and I ſaid that theſe times were as the conducting power of the body inverſely; but in the Experiments of which I am now about to give an account, I have in general reverſed the operation; that is to ſay, inſtead of obſerving the times of heating, I have firſt heated the body in boiling water, and then plunging it into a mixture of pounded ice and ice-cold water, I have noted the times taking up in *cooling*.

I have preferred this laſt method to the former, not only on account of the greater eaſe and convenience with which a thermometer, plunged into a mixture of water, may be obſerved, than when placed in a veſſel of boiling water, and ſurrounded by hot ſteam, but alſo on account of the greater accuracy of the Experiment: For the heat of boiling water varying with the variations of the preſſure of the atmoſphere, the Experiments made upon different days will have different reſults, and of courſe, ſtrictly ſpeaking, cannot be compared together; but the temperature of pounded ice and water is ever the ſame, and of courſe the reſults of the Experiments are uniform.

In heating the thermometer, I did not in general bring it to the temperature of the boiling water, becauſe this temperature, as I have juſt obſerved, is variable; but when the mercury had attained the 75° of its ſcale, I immediately took it out of the boiling water, and plunged it into the ice and water; or, which I take to be ſtill more accurate, ſuffering the mercury to riſe a degree or two above 75°, and then taking it out of the boiling water, I held it over the veſſel containing the pounded ice and water, ready to plunge it into that mixture the moment the mercury, deſcending, paſſed the 75°.

Having a watch at my ear which beat half ſeconds (which I counted), I noted the time of the paſſage of the mercury over the diviſions of the thermometer, marking 70° and every tenth degree from it, deſcending to 10° of the ſcale. I continued the cooling to 0°, or the temperature of the ice and water, in very few inſtances; as this took up much time, and was attended with no particular advantage, the determination of the times taking up in cooling 60 degrees of Reaumur's ſcale, that is to ſay, from 70° to 10°, being quite ſufficient to aſcertain the conducting power of any body whatever.

During the time of cooling in ice and water, the thermometer was conſtantly moved about in this mixture from one place to another; and there was always ſo much pounded ice mixed with the water, that the ice appeared above the ſurface of the water; the veſſel, which was a large earthen jar, being firſt quite filled with pounded ice, and the water being afterwards poured upon it, and freſh quantities of pounded ice being added as the occaſion required.

Having deſcribed the apparatus made uſe of in theſe Experiments, and the manner of performing the different operations, I ſhall now proceed to give an account of the Experiments themſelves.

My firſt attempt was to diſcover the relative conducting powers of ſuch ſubſtances as are commonly made uſe of for clothing; accordingly, having procured a quantity of *raw ſilk*, as ſpun by the worm; *ſheep's wool*; *cotton wool*; *linen* in the form of the fineſt lint, being the ſcrapings of very fine Iriſh linen; the fineſt part of the *fur of the beaver*, ſeparated from the ſkin, and from the long hair; the fineſt part of the *fur of a white Ruſſian hare;* and

Eider down; I introduced ſucceſſively 16 grains in weight of each of theſe ſubſtances into the globe of the paſſage-thermometer, and placing it carefully and equally round the bulb of the thermometer, I heated the thermometer in boiling water, as before deſcribed, and taking it out of the boiling water, plunged it into pounded ice and water, and obſerved the times of cooling.

But as the interſtices of theſe bodies thus placed in the globe were filled with air, I firſt made the Experiment with air alone, and took the reſult of that Experiment, as a ſtandard by which to compare all the others; the reſults of three Experiments with air were as follow:

The bulb of the thermometer ſurrounded by air.

Heat loſt.	Exp. No. 1.	Exp. No. 2.	Heat acquired.	Exp. No. 3.
	Time elapſed.	Time elapſed.		Time elapſed.
70°	—	—	10°	—
60°	38″	38″	20°	39″
50°	46	46	30°	43
40°	59	59	40°	53
30°	80	79	50°	67
20°	122	122	60°	96
10°	231	230	70°	175
Total times.	576	574	—	473

The following Table ſhows the reſults of the Experiments, with the various ſubſtances therein mentioned:

Heat loſt.	Air.	Raw ſilk, 16 grs.	Sheep's wool, 16 grs.	Cotton wool, 16 grs.	Fine lint, 16 grs.	Beaver's fur, 16 grs.	Hare's fur, 16 grs.	Eider down, 16 grs.
	Exp. 1.	Exp. 4.	Exp. 5.	Exp. 6.	Exp. 7.	Exp. 8.	Exp. 9.	Ex. 10.
70°	—	—	—	—	—	—	—	—
60°	38′	94″	79″	83″	80″	99″	97″	98″
50°	46	110	95	95	93	116	117	116
40°	59	133	118	117	115	153	144	146
30°	80	185	162	152	150	185	193	192
20°	122	273	238	221	218	265	270	268
10°	231	489	426	378	376	478	494	485
Total times.	576	1284	1118	1046	1032	1296	1315	1305

Now the *warmth* of a body, or its power to confine Heat, being as its power of reſiſting the paſſage of Heat through it, (which I ſhall call its *non-conducting power*,) and the time taken up by any body in cooling, which is ſurrounded by any medium through which the Heat is obliged to paſs, being, *cæteris paribus*, as the reſiſtance which the medium oppoſes to the paſſage of the Heat, it appears that the *warmth* of the bodies mentioned in the foregoing Table are as the times of cooling; the *conducting powers* being inverſely as thoſe times, as I have formerly ſhown.

From the reſults of the foregoing Experiments it appears, that of the ſeven different ſubſtances made uſe of, *hare's fur* and *Eider down* were the warmeſt; after theſe came beaver's fur; raw ſilk; ſheep's wool; cotton wool; and laſtly, lint, or the ſcrapings of fine linen; but I acknowledge that the differences in the warmth of theſe ſubſtances were much leſs than I expected to have found them.

Suſpecting that this might ariſe from the volumes or ſolid contents of the ſubſtances being different, (though their weights were the ſame,) ariſing from the difference of their ſpecific gravities; and as it was not eaſy to determine the ſpecific gravities of theſe ſubſtances with accuracy; in order to ſee how far any known difference in the volume or quantity of the ſame ſubſtance, confined always in the ſame ſpace, would add to or diminiſh the time of cooling, or to the apparent warmth of the covering, I made the three following Experiments.

In the firſt, the bulb of the thermometer was ſurrounded by 16 grains of Eider down; in the ſecond by 32 grains; and in the third by 64 grains; and in all theſe Experiments the ſubſtance was made to occupy exactly the ſame ſpace, viz. the whole internal capacity of the glaſs globe, in the centre of which the bulb of the thermometer was placed; conſequently the thickneſs of the covering of the thermometer remained the ſame, while its denſity was varied in proportion to the numbers 1, 2, and 4.

The reſults of theſe Experiments were as follow:

The bulb of the thermometer being ſurrounded by Eider down.

Heat loſt.	16 grains.	32 grains.	64 grains.
	(Exp. No. 11.)	(Exp. No. 12.)	(Exp. No. 13.)
70°	—	—	—
60°	97″	111″	112″
50°	117	128	130
40°	145	157	165
30°	192	207	224
20°	267	304	326
10°	486	565	658
Total times.	1304	1472	1615

Without ſtopping at preſent to draw any particular concluſions from the reſults of theſe Experiments, I ſhall proceed to give an account of ſome others, which will afford us a little further inſight into the nature of ſome of the circumſtances upon which the warmth of covering depends.

Finding, by the laſt Experiments, that the denſity of the covering added ſo conſiderably to the warmth of it, its thickneſs remaining the ſame, I was now deſirous of diſcovering how far the internal ſtructure of it contributed to render it more or leſs pervious to Heat, its thickneſs and quantity of matter remaining the ſame. By internal ſtructure, I mean the diſpoſition of the parts of the ſubſtance which forms the covering; thus they may be extremely divided, or very fine, as raw ſilk as ſpun by the worms, and they may be equally diſtributed through the whole ſpace they occupy; or they may be coarſer, or in larger maſſes, with larger interſtices, as the ravelings of cloth, or cuttings of threads.

If Heat paſſed *through* the ſubſtances made uſe of for covering, and if the warmth of the covering depended ſolely upon the difficulty which the Heat meets with in its paſſage through the ſubſtances, *or ſolid parts*, of which they are compoſed; in that caſe, the warmth of covering would be always, *cæteris paribus*, as the quantity of materials of which it is compoſed; but that this is not the caſe, the following, as well as the foregoing Experiments clearly evince.

Having, in the Experiment N° 4, aſcertained the warmth of 16 grains of raw ſilk, I now repeated the Experiment with the ſame quantity, or weight, of the ravelings of white taffety, and afterwards with a like quantity of common ſewing ſilk, cut into lengths of about two inches.

The following Table ſhows the reſults of theſe three Experiments:

Heat loſt.	Raw ſilk, 16 grs.	Ravelings of taffety, 16 grs.	Sewing ſilk cut into lengths, 16 grs.
	Exp. 4.	Exp. 14.	Exp. 15.
70°	—	—	—
60°	94″	90″	67″
50°	110	106	79
40°	133	128	99
30°	185	172	135
20°	273	246	195
10°	489	427	342
Total times.	1284	1169	917

Here, notwithſtanding that the quantities of the ſilk were the ſame in the three Experiments, and though in each of them it was made to occupy the ſame ſpace, yet the warmth of the coverings which were formed were very different, owing to the different diſpoſition of the material.

The raw ſilk was very fine, and was very equally diſtributed through the ſpace it occupied, and it formed a warm covering.

The ravelings of taffety were alſo fine, but not ſo fine as the raw ſilk, and of courſe the interſtices between its threads were greater, and it was leſs warm; but the cuttings of ſewing ſilk were very coarſe, and conſequently it was very unequally diſtributed in the ſpace in which it was confined; and it made a very bad covering for confining Heat.

It is clear from the reſults of the five laſt Experiments, that the air which occupies the interſtices of bodies, made uſe of for covering, acts a very important part in the operation of confining Heat; yet I ſhall poſtpone the examination of that circumſtance till I ſhall have given an account of ſeveral other Experiments, which, I think, will throw ſtill more light upon that ſubject.

But, before I go any further, I will give an account of three Experiments which I made, or rather the ſame Experiment which I repeated three times the ſame day, in order to ſee how far Experiments of this kind may be depended on, as being regular in their reſults.

The glaſs globe of the paſſage-thermometer being filled with 16 grains of cotton-wool, the inſtrument was heated and cooled three times ſucceſſively, when the times of cooling were obſerved as follows:

Heat loſt.	Exp. 16.	Exp. 17.	Exp. 18.
70°	—	—	—
60°	82″	84″	83″
50°	96	95	95
40°	118	117	116
30°	152	153	151
20°	221	221	220
10°	380	377	377
Total times.	1049	1047	1042

The difference of the times of cooling in theſe three Experiments was extremely ſmall; but regular as theſe Experiments appear to have been in their reſults, they were not more ſo than the other Experiments made in the ſame way, many of which were repeated two or three times, though, for the ſake of brevity, I have put them down as ſingle Experiments.

But to proceed in the account of my inveſtigations relative to the cauſes of the warmth of warm clothing. Having found that the fineneſs and equal diſtribution of a body or ſubſtance made uſe of to form a covering to confine Heat, contributes ſo much to the warmth of the covering, I was deſirous, in the next place, to ſee the effect of *condenſing* the covering, its quantity of matter remaining the ſame, but its thickneſs being diminiſhed in proportion to the increaſe of its denſity.

The Experiment I made for this purpoſe was as follows:—I took 16 grains of common ſewing ſilk, neither very fine nor very coarſe, and winding it about the bulb of the thermometer in ſuch a manner that it entirely covered it, and was as nearly as poſſible of the ſame thickneſs in every part, I replaced the thermometer in its cylinder and globe, and heating it in boiling water, cooled it in ice and water, as in the foregoing Experiments. The reſult of the Experiment may be ſeen in the following Table; and in order that it may be compared with thoſe made with the ſame quantity of ſilk differently diſpoſed of, I have placed thoſe Experiments by the ſide of it:

Heat loſt.	Raw ſilk, 16 grs.	Fine ravelings of taffety, 16 grs.	Sewing ſilk cut into lengths, 16 grs.	Sewing ſilk, 16 grs. wound round the bulb of the thermometer.
	Exp. No. 4.	Exp. No. 14.	Exp. No. 15.	Exp. No. 19.
70°	—	—	—	—
60°	94″	90″	67″	46″
50°	110	106	79	62
40°	133	128	99	85
30°	185	172	135	121
20°	273	246	195	191
10°	489	427	342	399
Total times.	1214	1169	917	904

It is not a little remarkable, that, though the covering formed of ſewing ſilk wound round the bulb of the thermometer in the 19th Experiment, appeared to have ſo little power of confining the Heat when the inſtrument was very hot, or when it was firſt plunged into the ice and water, yet afterwards, when the Heat of the thermometer approached much nearer to that of the ſurrounding medium, its power of confining the Heat which remained in the bulb of the thermometer appeared to be even greater than that of the ſilk in the Experiment N° 15, the time of cooling from 20° to 10° being in the one 399″, and in the other 342″. The ſame appearance was obſerved in the following Experiments, in which the bulb of the thermometer was ſurrounded by threads of *wool*, of *cotton*, and of *linen*, or *flax*, wound round it, in the like manner as the ſewing ſilk was wound round it in the laſt Experiment.

The following Table ſhows the reſults of theſe Experiments, with the threads of various kinds; and that they may the more eaſily be compared with thoſe made with the ſame quantity of the ſame ſubſtances in a different form, I have placed the accounts of theſe Experiments by the ſide of each other. I have alſo added the account of an Experiment, in which 16 grains of fine linen cloth were wrapped round the bulb of the thermometer, going round it nine times, and being bound together at the top and bottom of it, ſo as completely to cover it.

Heat loſt.	*Sheep's wool*, 16 grains, ſurrounding the bulb of the thermometer.	*Woollen thread*, 16 grains, wound round the bulb of the thermometer.	*Cotton wool*, 16 grains, ſurrounding the bulb of the thermometer.	*Cotton thread*, 16 grains, wound round the bulb of the thermometer.	*Lint*, 16 grains, ſurrounding the bulb of the thermometer.	*Linen thread*, 16 grains, wound round the bulb of the thermometer.	*Linen cloth*, 16 grains, wrapped round the bulb of the thermometer.
	Exp. 5.	Ex. 20.	Exp. 6.	Ex. 21.	Exp. 7.	Ex. 22.	Exp. 23.
70°	—	—	—	—	—	—	—
60°	79″	46″	83″	45″	80	46″	42″
50°	95	63	95	60	93	62	56
40°	118	89	117	83	115	83	74
30°	162	126	152	115	150	117	108
20°	238	200	221	179	218	180	168
10°	426	410	378	370	376	385	338
Total times.	1118	934	1046	852	1032	873	783

That thread wound light round the bulb of the thermometer ſhould form a covering leſs warm than the ſame quantity of wool, or other raw materials of which the thread is made, ſurrounding the bulb of the thermometer in a more looſe manner, and conſequently occupying a greater ſpace, is no more than what I expected, from the idea I had formed of the cauſes of the warmth of covering; but I confeſs I was much ſurpriſed to find that there is ſo great a difference in the relative warmth of theſe two coverings, when they are employed to confine great degrees of Heat, and when the Heat they confine is much leſs in proportion to the temperature of the ſurrounding medium. This difference was very remarkable; in the Experiments with ſheep's wool, and with woollen thread, the warmth of the covering formed of 16 grains of the former, was to that formed of 16 grains of the latter, when the bulb of the thermometer was heated to 70° and cooled to 60°, as 79 to 46 (the ſurrounding medium being at 0°); but afterwards, when the thermometer had only fallen from 20° to 10° of Heat, the warmth of the wool was to that of the woollen thread only as 426 to 410; and in the Experiments with lint, and with linen thread, when the Heat was much abated, the covering of the thread appeared to be even warmer than that of the lint, though in the beginning of the Experiments, when the Heat was much greater, the lint was warmer than the thread, in the proportion of 80 to 46.

From hence it ſhould ſeem that a covering may, under certain circumſtances, be very good for confining ſmall degrees of warmth, which would be but very indifferent when made uſe of for confining a more intenſe Heat, and *vice verſa*. This, I believe, is a new fact; and, I think the knowledge of it may lead to further diſcoveries relative to the cauſes of the warmth of coverings, or the manner in which Heat makes its paſſage through them. But I forbear to enlarge upon this ſubject, till I ſhall have given an account of ſeveral other Experiments, which I think throw more light upon it, and which will conſequently render the inveſtigation eaſier and more ſatisfactory.

With a view to determine how far the power which certain bodies appear to poſſeſs of confining Heat, when made uſe of as covering, depends upon the natures of thoſe bodies, conſidered as chymical ſubſtances, or upon the chymical principles of which they are compoſed, I made the following Experiments.

As *charcoal* is ſuppoſed to be compoſed almoſt entirely of phlogiſton *, I thought that, if that principle was the cauſe either of the conducting power, or the non-conducting power of the bodies which contain it, I ſhould diſcover it by making the Experiment with charcoal, in the manner as I had done with various other bodies. Accordingly, having filled the globe of the paſſage-thermometer with 176 grains of that ſubſtance in very fine powder, (it having been pounded in a mortar, and ſifted through a fine ſieve,) the bulb of the thermometer being ſurrounded by this powder, the inſtrument was heated in boiling water, and being afterwards plunged into a mixture of pounded ice and water, the times of cooling were obſerved as mentioned in the following Table. I afterwards repeated the Experiment with lampblack, and with very pure and very dry wood aſhes; the reſults of which Experiments were as under-mentioned:

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* This was written in the year 1787, when *Phlogiſton* was by many ſuppoſed to have a real exiſtence.

	The bulb of the thermometer ſurrounded by			
Heat loſt.	176 grains of fine powder of charcoal.	176 grains of fine powder of charcoal.	195 grains of lampblack.	307 grains of pure dry wood aſhes.
	Exp. No. 24.	Exp. No. 25.	Exp. No. 26.	Exp. No. 27.
70°	—	—	—	—
60°	79″	91″	124″	96″
50°	95	91	118	92
40°	100	109	134	107
30°	139	133	164	136
20°	196	192	237	185
10°	331	321	394	311
Total times.	940	937	1171	927

The Experiment N° 25 was ſimply a repetition of that numbered 24, and was made immediately after it; but, in moving the thermometer about in the former Experiment, the powder of charcoal which filled the globe was ſhaken a little together, and to this circumſtance I attribute the difference in the reſults of the two Experiments.

In the Experiments with lampblack, and with wood aſhes, the times taken up in cooling from 70° to 60° were greater than thoſe employed in cooling from 60° to 50°; this moſt probably aroſe from the conſiderable quantity of Heat contained by theſe ſubſtances, which was firſt to be diſpoſed of, before they could receive and communicate to the ſurrounding medium that which was contained by the bulb of the thermometer.

The next Experiment I made was with *ſemen lycopodii*, commonly called witch-meal, a ſubſtance which poſſeſſes very extraordinary properties. It is almoſt impoſſible to wet it; a quantity of it ſtrewed upon the ſurface of a baſin of water, not only ſwims upon the water without being wet, but it prevents other bodies from being wet which are plunged into the water through it; ſo that a piece of money, or other ſolid body, may be taken from the bottom of the baſin by the naked hand, without wetting the hands; which is one of the tricks commonly ſhown by the jugglers in this country (Bavaria): this meal covers the hand, and deſcending along with it to the bottom of the baſin, defends it from the water. This ſubſtance has the appearance of an exceeding fine, light, and very moveable yellow powder, and it is very inflammable; ſo much ſo, that being blown out of a quill into the flame of a candle, it flaſhes like gunpowder, and it is made uſe of in this manner in our theatres for imitating lightning.

Conceiving that there muſt have been a ſtrong attraction between this ſubſtance and air, and ſuſpecting, from ſome circumſtances attending ſome of the foregoing Experiments, that the warmth of a covering depends not merely upon the fineneſs of the ſubſtance of which the covering is formed, and the diſpoſition of its parts, but that it ariſes in ſome meaſure from a certain attraction between the ſubſtance and the air which fills its interſtices, I thought that an Experiment with *ſemen lycopodii* might poſſibly throw ſome light upon this matter; and in this opinion I was not altogether miſtaken, as will appear by the reſults of the three following Experiments.

The bulb of the thermometer ſurrounded by 256 grs. of *ſemen lycopodii.*

Heat loſt.	Cooled.	Cooled.	Heat acquired.	Heated.
	Exp. No. 28.	Exp. No. 29.		Exp. No. 30.
70°	—	—	0°	—
60°	146″	157″	10°	230″
50°	162	160	20°	68
40°	175	170	30°	63
30°	209	203	40°	70
20°	284	288	50°	121
10°	502	513	60°	316
—	—	—	70°	1585
Total times.	1478	1491	—	2459

In the laſt Experiment (N° 30) the reſult of which was ſo very extraordinary, the inſtrument was cooled to 0° in thawing ice, after which it was plunged ſuddenly into boiling water, where it remained till the incloſed thermometer had acquired the Heat of 70°, which took up no leſs than 2456 ſeconds, or above 40 minutes; *and it had remained* *in the boiling water full a minute and an half before the mercury in the thermometer ſhowed the leaſt ſign of riſing.* Having at length been put into motion, it roſe very rapidly 40 or 50 degrees, after which its motion gradually abating became ſo ſlow, that it took up 1585 ſeconds, or ſomething more than 26 minutes, in riſing from 60° to 70°, though the temperature of the medium in which it was placed during the whole of this time was very nearly 80°; the mercury in the barometer ſtanding but little ſhort of 27 Paris inches.

All the different ſubſtances which I had yet made uſe of in theſe Experiments for ſurrounding or covering the bulb of the thermometer, fluids excepted, had, in a greater, or in a leſs degree confined the Heat, or prevented its paſſing into or out of the thermometer ſo rapidly as it would have done, had there been nothing but air in the glaſs globe, in the centre of which the bulb of the thermometer was ſuſpended. But the great queſtion is, how, or in what manner, they produced this effect?

And firſt, it was not in conſequence of their own non-conducting powers, ſimply conſidered; for, if inſtead of being only bad conductors of Heat, we ſuppoſe them to have been totally impervious to Heat, their volumes or ſolid contents were ſo exceedingly ſmall in proportion to the capacity of the globe in which they were placed, that, had they had no effect whatever upon the air filling their interſtices, that air would have been ſufficient to have conducted all the Heat communicated, in leſs time than was actually taken up in the Experiment.

The diameter of the globe being 1,6 inches, its contents amounted to 2,14466 cubic inches; and the contents of the bulb of the thermometer being only 0,08711 of a cubic inch, (its diameter being 0,55 of an inch,) the ſpace between the bulb of the thermometer and the internal ſurface of the globe amounted to 2,14466—0,08711=2,05755 cubic inches; the whole of which ſpace was occupied by the ſubſtances by which the bulb of the thermometer was ſurrounded in the Experiments in queſtion.

But though theſe ſubſtances occupied this ſpace, they were far from *filling it;* by much the greater part of it being filled by the air which occupied the interſtices of the ſubſtances in queſtion. In the Experiment N° 4, this ſpace was occupied by 16 grains of raw ſilk; and as the ſpecific gravity of raw ſilk is to that of water as 1734 to 1000, the volume of this ſilk was equal to the volume of 9,4422 grains of water; and as 1 cubic inch of water weighs 253,185 grains, its volume was equal to $\frac{9,4422}{253,1850}$ =0,037294 of a cubic inch; and, as the ſpace it occupied amounted to 2,05755 cubic inches, it appears that the ſilk filled no more than about $\frac{1}{55}$ part of the ſpace in which it was confined, the reſt of that ſpace being filled with air.

In the Experiment N° 1, when the ſpace between the bulb of the thermometer and the glaſs globe, in the centre of which it was confined, was filled with nothing but air, the time taken up by the thermometer in cooling from 70° to 10° was

576 ſeconds; but in the Experiment N° 4, when this ſame ſpace was filled with 54 parts air, and 1 part raw ſilk, the time of cooling was 1284 ſeconds.

Now, ſuppoſing that the ſilk had been totally incapable of conducting any Heat at all, if we ſuppoſe, at the ſame time, that it had no power to prevent the air remaining in the globe from conducting it, in that caſe its preſence in the globe could only have prolonged the time of cooling in proportion to the quantity of the air it had diſplaced to the quantity remaining, that is to ſay, as 1 is to 54, or a little more than 10 ſeconds. But the time of cooling was actually prolonged 708 ſeconds (for in the Experiment N° 1, it was 576 ſeconds, and in the Experiment N° 4, it was 1284 ſeconds, as has juſt been obſerved); and this ſhows, that the ſilk not only did not conduct the Heat itſelf, but that it prevented the air by which its interſtices were filled from conducting it; or, at leaſt, it greatly weakened its power of conducting it.

The next queſtion which ariſes is, how air can be *prevented* from conducting Heat? and this neceſſarily involves another, which is, *how does air conduct Heat?*

If air conducted Heat, as it is probable that the metals, and water, and all other ſolid bodies and unelaſtic fluids conduct it, that is to ſay, if its particles remaining in their places, the Heat paſſed from one particle to another, through the whole maſs,—as there is no reaſon to ſuppoſe that the propagation of Heat is neceſſarily in right lines,—I cannot conceive how the interpoſition of ſo ſmall a quantity of any ſolid body as $\frac{1}{55}$ part of the volume of the air could have effected ſo remarkable a diminution of the conducting power of the air, as appeared in the Experiment (N° 4) with raw ſilk, above mentioned.

If air and water conducted Heat in the ſame *manner*, it is more than probable that their conducting powers might be impaired by the ſame means; but when I made the Experiment with water, by filling the glaſs globe, in the centre of which the bulb of the thermometer was ſuſpended, with that fluid, and afterwards varied the Experiment, by adding 16 grains of raw ſilk to the water, I did not find that the conducting power of the water was ſenſibly impaired by the preſence of the ſilk *.

But we have juſt ſeen that the ſame ſilk, mixed with an equal volume of air, diminiſhed its conducting power in a very remarkable degree; conſequently, there is great reaſon to conclude that water and air conduct Heat in a *different manner*.

But the following Experiment, I think, puts the matter beyond all doubt.

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* The Experiment here mentioned was made in the year 1787; but the reſult of a more careful inveſtigation of the ſubject has ſince ſhown that Heat is not propagated in water in the manner here ſuppoſed. (See Eſſay VII.)

It is well known, that the power which air poſſeſſes of holding water in ſolution is augmented by Heat, and diminiſhed by cold, and that, if hot air is ſaturated with water, and if this air is afterwards cooled, a part of its water is neceſſarily depoſed.

I took a cylindrical bottle of very clear tranſparent glaſs, about 8 inches in diameter, and 12 inches high, with a ſhort and narrow neck, and ſuſpending a ſmall piece of linen rag, moderately wet, in the middle of it, I plunged it into a large veſſel of water, warmed to about 100° of Fahrenheit's thermometer, where I ſuffered it to remain till the contained air was not only warm, but thoroughly ſaturated with the moiſture which it attracted from the linen rag, the mouth of the bottle being well ſtopped up during this time with a good cork; this being done, I removed the cork for a moment, to take away the linen rag, and ſtopping up the bottle again immediately, I took it out of the warm water, and plunged it into a large cylindrical jar, about 12 inches in diameter, and 16 inches high, containing juſt ſo much ice-cold water, that, when the bottle was plunged into it, and quite covered by it, the jar was quite full.

As the jar was of very fine tranſparent glaſs, as well as the bottle, and as the cold water contained in the jar was perfectly clear, I could ſee what paſſed in the bottle moſt diſtinctly; and having taken care to place the jar upon a table near the window, in a very favourable light, I ſet myſelf to obſerve the appearances which ſhould take place, with all that anxious expectation which a conviction that the reſult of the Experiment muſt be deciſive, naturally inſpired.

I was certain, that the air contained in the bottle could not part with its Heat, without at the ſame time, that is to ſay, *at the ſame moment*, and *in the ſame place*, parting with a portion of its water; if, therefore, the Heat penetrated the maſs of air from the centre to the ſurface, or *paſſed through it* from particle to particle, in the ſame manner as it is probable that it paſſes through water, and all other unelaſtic fluids*, by far the greateſt part of the air contained in the bottle would part with its Heat, when *not actually in contact with the glaſs*, and a proportional part of its water being let fall at the ſame time, and in the *ſame place*, would neceſſarily deſcend in the form of rain; and, though this rain might be too fine to be viſible in its deſcent, yet I was ſure I ſhould find it at the bottom of the bottle, if not in viſible drops of water, yet in that kind of cloudy covering which cold glaſs acquires from a contact with hot ſteam or watery vapour.

But if the particles of air, inſtead of communicating their Heat from one to another, from the centre to the ſurface of the bottle, each in its turn, and for itſelf, came to the ſurface of the bottle, and there depoſited its Heat and its water, I concluded that the cloudineſs occaſioned by this depoſit of water would appear all over the bottle, or, at leaſt, not more of it at the bottom than at the ſides, but rather leſs; and this I found to be the caſe in fact.

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* This opinion reſpecting the manner in which Heat is propagated in water, and other unelaſtic fluids, was afterwards found to be erroneous, as has been ſhewn in the preceding Eſſay.

The cloudineſs firſt made its appearance upon the *ſides* of the bottle, *near the top of it;* and from thence it gradually ſpread itſelf downwards, till, growing fainter as it deſcended lower, it was hardly viſible at the diſtance of half an inch from the bottom of the bottle; and *upon the bottom itſelf*, which was nearly flat, *there was ſcarcely the ſmalleſt appearance of cloudineſs.*

Theſe appearances, I think, are eaſy to be accounted for. The air immediately in contact with the glaſs being cooled, and having depoſited a part of its water upon the ſurface of the glaſs, at the ſame time that it communicates to it its Heat, ſlides downwards by the ſides of the bottle in conſequence of its increaſed ſpecific gravity, and, taking its place at the bottom of the bottle, forces the whole maſs of hot air upwards; which, in its turn coming to the ſides of the bottle, *there* depoſits its Heat and its water, and afterwards bending its courſe downwards, this circulation is continued till all the air in the bottle has acquired the exact temperature of the water in the jar.

From hence it is clear why the firſt appearance of condenſed vapour is near the top of the bottle, as alſo why the greateſt collection of vapour is in that part, and that ſo very ſmall a quantity of it is found nearer the bottom of the bottle.

This Experiment confirmed me in an opinion which I had for ſome time entertained, that, though the particles of air, individually, or each for itſelf, are capable of receiving and *tranſporting* Heat, yet air in a quieſcent ſtate, or as a fluid whoſe parts are at reſt with reſpect to each other, is not capable of conducting it, or giving it a paſſage; in ſhort, that Heat is incapable of *paſſing through a maſs of air*, penetrating from one particle of it to another, and that it is to this circumſtance that its non-conducting power is principally owing.

It is alſo to this circumſtance, in a great meaſure, that it is owing that its non-conducting power, or its apparent warmth when employed as a covering for confining Heat, is ſo remarkably increaſed upon its being mixed with a ſmall quantity of any very fine, light, ſolid ſubſtance, ſuch as the raw ſilk, fur, Eider down, &c. in the foregoing Experiments: for, as I have already obſerved, though theſe ſubſtances, in the very ſmall quantities in which they were made uſe of, could hardly have prevented, in any conſiderable degree, the air from conducting, or giving a *paſſage* to the Heat, had it been capable of paſſing through it, yet they might very much impede it in the operation of tranſporting it.

But there is another circumſtance which it is neceſſary to take into the account, and that is the attraction which ſubſiſts between air and the bodies above-mentioned, and other like ſubſtances, conſtituting natural and artificial clothing: for, though the incapacity of air to give a paſſage to Heat in the manner ſolid bodies permit it to paſs through them, may enable us to account for its warmth under certain circumſtances, yet the bare admiſſion of this principle does not ſeem to be ſufficient to account for the very extraordinary degrees of warmth which we find in furs and in feathers, and in various other kinds of natural and artificial clothing; nor even that which we find in ſnow; for if we ſuppoſe the particles of air to be at liberty to *carry off* the Heat which theſe bodies are meant to confine, without any other obſtruction or hindrance than that ariſing from their *vis inertiæ*, or the force neceſſary to put them in motion, it ſeems probable that the ſucceſſion of freſh particles of cold air, and the conſequent loſs of Heat, would be much more rapid than we find it to be in fact.

That an attraction, and a very ſtrong one, actually ſubſiſts between the particles of air, and the fine hair or furs of beaſts, the feathers of birds, wool, &c. appears by the obſtinacy with which theſe ſubſtances retain the air which adheres to them, even when immerſed in water, and put under the receiver of an air pump; and that this attraction is eſſential to the warmth of theſe bodies, I think is very eaſy to be demonſtrated.

In furs, for inſtance, the attraction between the particles of air, and the fine hairs in which it is concealed, being greater than the increaſed elaſticity,

ticity, or repulſion of thoſe particles with regard to each other, ariſing from the Heat communicated to them by the animal body, the air in the fur, though heated, is not eaſily diſplaced; and this coat of confined air is the real barrier which defends the animal body from the external cold. This air cannot *carry off* the Heat of the animal, becauſe it is itſelf confined, by its attraction to the hair or fur; and it tranſmits it with great difficulty, if it tranſmits at all, as has been abundantly ſhewn by the foregoing Experiments.

Hence it appears why thoſe furs which are the fineſt, longeſt, and thickeſt, are likewiſe the warmeſt; and how the furs of the *beaver*, of the *otter*, and of other like quadrupeds which live much in water, and the feathers of *water-fowls*, are able to confine the Heat of thoſe animals in winter, notwithſtanding the extreme coldneſs and great conducting power of the water in which they ſwim. The attraction between theſe ſubſtances, and the air which occupies their interſtices, is ſo great, that this air is not diſlodged even by the contact of water, but remaining in its place, it defends the body of the animal at the ſame time from being wet, and from being robbed of its Heat by the ſurrounding cold fluid; and it is poſſible that the preſſure of this fluid upon the covering of air confined in the interſtices of the fur, or feathers, may at the ſame time increaſe its warmth, or non-conducting power, in ſuch a manner that the animal may not, in fact, loſe more Heat when in water, than when in air: for we have ſeen by the foregoing Experiments, that, under certain circumſtances, the warmth of a covering is increaſed, by bringing its component parts nearer together, or by increaſing its denſity even at the expence of its thickneſs. But this point will be further inveſtigated hereafter.

Bears, wolves, foxes, hares, and other like quadrupeds, inhabitants of cold countries, which do not often take the water, have their fur much thicker upon their backs than upon their bellies. The heated air occupying the interſtices of the hairs of the animal tending naturally *to riſe upwards*, in conſequence of its increaſed elaſticity, would eſcape with much greater eaſe from the backs of quadrupeds than from their bellies, had not Providence wiſely guarded againſt this evil by increaſing the obſtructions in thoſe parts, which entangle it and confine it to the body of the animal. And this, I think, amounts almoſt to a proof of the principles aſſumed relative to the manner in which Heat is carried off by air, and the cauſes of the non-conducting power of air, or its apparent warmth, when, being combined with other bodies, it acts as a covering for confining Heat.

The ſnows which cover the ſurface of the earth in winter, in high latitudes, are doubtleſs deſigned by an all-provident Creator as a *garment* to defend it againſt the piercing winds from the polar regions, which prevail during the cold ſeaſon.

Theſe winds, notwithſtanding the vaſt tracts of continent over which they blow, retain their ſharpneſs as long as the ground they paſs over is covered with ſnow; and it is not till meeting with the ocean, they acquire, from a contact with its waters, the Heat which the ſnows prevent their acquiring from the earth, that the edge of their coldneſs is taken off, and they gradually die away and are loſt.

The winds are always found to be much colder when the ground is covered with ſnow than when it is bare, and this extraordinary coldneſs is vulgarly ſuppoſed to be communicated to the air by the ſnow; but this is an erroneous opinion; for theſe winds are in general much colder than the ſnow itſelf.

They retain their coldneſs, becauſe the ſnow prevents them from being warmed at the expence of the earth; and this is a ſtriking proof of the uſe of the ſnows in preſerving the Heat of the earth during the winter in cold latitudes.

It is remarkable that theſe winds ſeldom blow from the poles directly towards the equator, but from the land towards the ſea. Upon the eaſtern coaſt of North America the cold winds come from the north-weſt; but upon the weſtern coaſt of Europe, they blow from the north-eaſt.

That they ſhould blow towards thoſe parts where they can moſt eaſily acquire the Heat they are in ſearch of, is not extraordinary; and that they ſhould gradually ceaſe and die away, upon being warmed by a contact with the waters of the ocean, is likewiſe agreeable to the nature and cauſes of their motion; and if I might be allowed a conjecture reſpecting the principal *uſe of the ſeas*, or the reaſon why the proportion of water upon the ſurface of our globe is ſo great, compared to that of the land, it is *to maintain a more equal temperature in the different climates*, by heating or cooling the winds which at certain periods blow from the great continents.

That cold winds actually grow much milder upon paſſing over the ſea, and that hot winds are refreſhed by a contact with its waters, is very certain; and it is equally certain that the winds from the ocean are, in all climates, much more temperate than thoſe which blow from the land.

In the iſlands of Great Britain and Ireland, there is not the leaſt doubt but the great mildneſs of the climate is entirely owing to their ſeparation from the neighbouring continent by ſo large a tract of ſea; and in all ſimilar ſituations, in every part of the globe, ſimilar cauſes are found to produce ſimilar effects.

The cold north-weſt winds, which prevail upon the coaſt of North America during the winter, ſeldom extend above 100 leagues from the ſhore, and they are always found to be leſs violent, and leſs piercing, as they are further from the land.

Theſe periodical winds from the continents of Europe and North America prevail moſt towards the end of the month of February, and in the month of March; and I conceive that they contribute very eſſentially towards bringing on an early ſpring, and a fruitful ſummer, particularly when they are very violent in the month of March, and if at that time the ground is well covered with ſnow. The whole atmoſphere of the polar regions being, as it were, tranſported into the ocean by theſe winds, is there warmed and ſaturated with water: and, a great accumulation of air upon the ſea being the neceſſary conſequence of the long continuance of theſe cold winds from the ſhore, upon their ceaſing the warm breezes from the ſea neceſſarily commence, and, ſpreading themſelves upon the land far and wide, aſſiſt the returning ſun in diſmantling the earth of the remains of her winter garment, and in bringing forward into life all the manifold beauties of the new-born year.

This warmed air which comes in from the ſea, having acquired its Heat from a contact with the ocean, is, of courſe, ſaturated with water; and hence the warm ſhowers of April and May, ſo neceſſary to a fruitful ſeaſon.

The *ocean* may be conſidered as *the great reſervoir and equalizer of Heat;* and its benign influences in preſerving a proper temperature in the atmoſphere operate in all ſeaſons and in all climates.

The parching winds from the land under the torrid zone are cooled by a contact with its waters, and, in return, the breezes from the ſea, which at certain hours of the day come in to the ſhores in almoſt all hot countries, bring with them refreſhment,

ment, and, as it were, new life and vigour both to the animal and vegetable creation, fainting and melting under the exceſſive Heats of a burning ſun. What a vaſt tract of country, now the moſt fertile upon the face of the globe, would be abſolutely barren and uninhabitable on account of the exceſſive Heat, were it not for theſe refreſhing ſea-breezes! And is it not more than probable, that the extremes of heat and of cold in the different ſeaſons in the temperate and frigid zones would be quite intolerable, were it not for the influence of the ocean in preſerving an equability of temperature?

And to theſe purpoſes the ocean is wonderfully well adapted, not only on account of the great power of water to abſorb Heat, and the vaſt depth and extent of the different ſeas (which are ſuch that one ſummer or one winter could hardly be ſuppoſed to have any ſenſible effect in heating or cooling this enormous maſs); but alſo on account of the continual circulation which is carried on in the ocean itſelf, by means of the currents which prevail in it. The waters under the torrid zone being carried by theſe currents towards the polar regions, are there cooled by a contact with the cold winds, and, having thus communicated their Heat to theſe inhoſpitable regions, return towards the equator, carrying with them refreſhment for thoſe parching climates.

The wiſdom and goodneſs of Providence have often been called in queſtion with regard to the diſtribution of land and water upon the ſurface of our globe, the vaſt extent of the ocean having been conſidered as a proof of the little regard that has been paid to man in this diſtribution. But, the more light we acquire reſpecting the real conſtitution of things, and the various uſes of the different parts of the viſible creation, the leſs we ſhall be diſpoſed to indulge ourſelves in ſuch frivolous criticiſms.

END OF THE EIGHTH ESSAY.

Fig. 1.

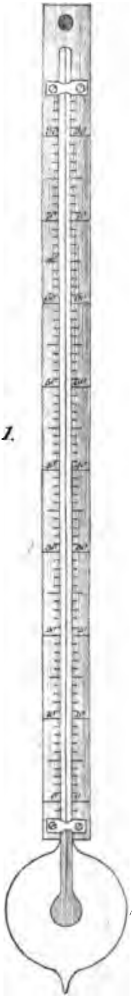


Fig. 2.

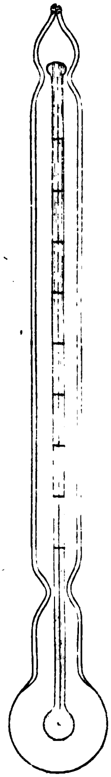


Fig. 3.

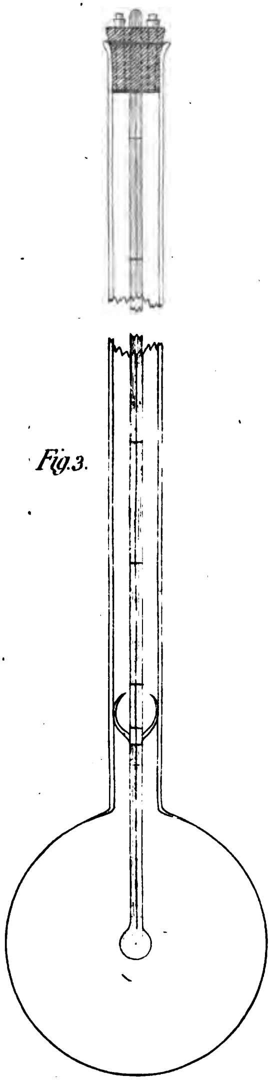
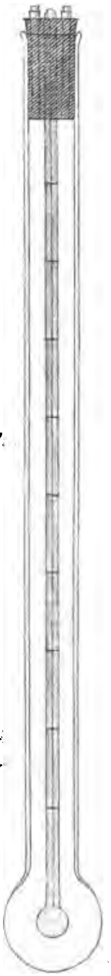


Fig. 4.



Scale of Inches

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ESSAY IX.

AN EXPERIMENTAL INQUIRY

CONCERNING

THE SOURCE OF THE HEAT WHICH IS EXCITED BY FRICTION.

[Read before the ROYAL SOCIETY, January 25, 1798.]

ESSAY IX.

An INQUIRY concerning the SOURCE of the HEAT which is EXCITED by FRICTION.

[Read before the ROYAL SOCIETY, January 25, 1798.]

IT frequently happens, that in the ordinary affairs and occupations of life, opportunities preſent themſelves of contemplating ſome of the moſt curious operations of Nature; and very intereſting philoſophical experiments might often be made, almoſt without trouble or expence, by means of machinery contrived for the mere mechanical purpoſes of the arts and manufactures.

I have frequently had occaſion to make this obſervation; and am perſuaded, that a habit of keeping the eyes open to every thing that is going on in the ordinary courſe of the buſineſs of life has oftener led, as it were by accident, or in the playful excurſions of the imagination, put into action by contemplating the moſt common appearances, to uſeful doubts, and ſenſible ſchemes for inveſtigation

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and improvement, than all the more intenſe meditations of philoſophers, in the hours expreſsly ſet apart for ſtudy.

It was by accident that I was led to make the Experiments of which I am about to give an account; and, though they are not perhaps of ſufficient importance to merit ſo formal an introduction, I cannot help flattering myſelf that they will be thought curious in ſeveral reſpects, and worthy of the honour of being made known to the Royal Society.

Being engaged, lately, in ſuperintending the boring of cannon, in the workſhops of the military arſenal at Munich, I was ſtruck with the very conſiderable degree of Heat which a braſs gun acquires, in a ſhort time, in being bored; and with the ſtill more intenſe Heat (much greater than that of boiling water, as I found by experiment) of the metallic chips ſeparated from it by the borer.

The more I meditated on theſe phænomena, the more they appeared to me to be curious and intereſting. A thorough inveſtigation of them ſeemed even to bid fair to give a farther inſight into the hidden nature of Heat; and to enable us to form ſome reaſonable conjectures reſpecting the exiſtence, or non-exiſtence, of an *igneous fluid*: a ſubject on which the opinions of philoſophers have, in all ages, been much divided.

In order that the Society may have clear and diſtinct ideas of the ſpeculations and reaſonings to which theſe appearances gave riſe in my mind, and alſo of the ſpecific objects of philoſophical inveſtigation they ſuggeſted to me, I muſt beg leave to ſtate them at ſome length, and in ſuch manner as I ſhall think beſt ſuited to anſwer this purpoſe.

From *whence comes* the Heat actually produced in the mechanical operation above mentioned?

Is it furniſhed by the metallic chips which are ſeparated by the borer from the ſolid maſs of metal?

If this were the caſe, then, according to the modern doctrines of latent Heat, and of caloric, the *capacity for Heat* of the parts of the metal, ſo reduced to chips, ought not only to be changed, but the change undergone by them ſhould be ſufficiently great to account for *all* the Heat produced.

But no ſuch change had taken place; for I found, upon taking equal quantities, by weight, of theſe chips, and of thin ſlips of the ſame block of metal ſeparated by means of a fine ſaw, and putting them, at the ſame temperature, (that of boiling water,) into equal quantities of cold water, (that is to ſay, at the temperature of 59°½ F.) the portion of water into which the chips were put was not, to all appearance, heated either leſs or more than the other portion, in which the ſlips of metal were put.

This Experiment being repeated ſeveral times, the reſults were always ſo nearly the ſame, that I could not determine whether any, or what change, had been produced in the metal, *in regard to its* *capacity for Heat*, by being reduced to chips by the borer*.

From hence it is evident, that the Heat produced could not poſſibly have been furniſhed at the expence of the latent Heat of the metallic chips. But, not being willing to reſt ſatisfied with theſe trials, however concluſive they appeared to me to be, I had recourſe to the following ſtill more deciſive Experiment:

Taking a cannon, (a braſs ſix-pounder,) caſt ſolid, and rough as it came from the foundry, (ſee Fig. 1. Tab. IV.) and fixing it (horizontally) in the machine uſed for boring, and at the ſame time finiſhing the outſide of the cannon by turning, (ſee Fig. 2.) I cauſed its extremity to be cut off; and, by turning down the metal in that part, a ſolid cylinder was formed, $7\frac{3}{4}$ inches in diameter, and $9\frac{8}{10}$ inches long; which, when finiſhed, remained joined to the reſt of the metal (that which, properly ſpeaking, conſtituted the cannon) by a ſmall cylindrical neck, only $2\frac{1}{5}$ inches in diameter, and $3\frac{8}{10}$ inches long.

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* As theſe Experiments are important, it may perhaps be agreeable to the Society to be made acquainted with them in their details.

One of them was as follows:

To 4590 grains of water, at the temperature of 59°½ F. (an allowance as compenſation, reckoned in water, for the capacity for Heat of the containing cylindrical tin veſſel, being included,) were added 1016⅛ grains of gun-metal in thin ſlips, ſeparated from the gun by means of a fine ſaw, being at the temperature of 210° F. When they had remained together 1 minute, and had been well ſtirred about, by means of a ſmall rod of light wood, the Heat of the mixture was found to be =63°.

From this Experiment, the *ſpecific Heat* of the metal, calculated according to the rule given by Dr. CRAWFORD, turns out to be =0.1100, that of water being =1.000.

An Experiment was afterwards made with the metallic chips, as follows:

To the ſame quantity of water as was uſed in the Experiment above mentioned, at the ſame temperature, (*viz.* 59°½,) and in the ſame cylindrical tin veſſel, were now put 1016⅛ grains of metallic chips of gun-metal, bored out of the ſame gun from which the ſlips uſed in the foregoing Experiment were taken, and at the ſame temperature (210°). The Heat of the mixture, at the end of 1 minute, was juſt 63°, as before; conſequently the ſpecific Heat of theſe metallic chips was =0.1100. Each of the above Experiments was repeated three times, and always with nearly the ſame reſults.

This ſhort cylinder, which was ſupported in its horizontal poſition, and turned round its axis, by means of the neck by which it remained united to the cannon, was now bored with the horizontal borer uſed in boring cannon; but its bore, which was 3.7 inches in diameter, inſtead of being continued through its whole length (9.8 inches) was only 7.2 inches in length; ſo that a ſolid bottom was left to this hollow cylinder, which bottom was 2.6 inches in thickneſs.

This cavity is repreſented by dotted lines in Fig. 2; as alſo in Fig. 3. where the cylinder is repreſented on an enlarged ſcale.

This cylinder being deſigned for the expreſs purpoſe of generating Heat *by friction*, by having a blunt borer forced againſt its ſolid bottom at the ſame time that it ſhould be turned round its axis by the force of horſes, in order that the Heat accumulated in the cylinder might from time to time be meaſured, a ſmall round hole, (ſee *d*, *e*, Fig. 3) 0.37 of an inch only in diameter, and 4.2 inches in

depth,

depth, for the purpoſe of introducing a ſmall cylindrical mercurial thermometer, was made in it, on one ſide, in a direction perpendicular to the axis of the cylinder, and ending in the middle of the ſolid part of the metal which formed the bottom of its bore.

The ſolid contents of this hollow cylinder, excluſive of the cylindrical neck by which it remained united to the cannon, were 385¼ cubic inches, Engliſh meaſure; and it weighed 113.13lb. Avoirdupois: as I found, on weighing it at the end of the courſe of Experiments made with it, and after it had been ſeparated from the cannon with which, during the Experiments, it remained connected*.

Experiment, N° 1.

This Experiment was made in order to aſcertain *how much Heat* was actually generated by friction, when a blunt ſteel borer being ſo forcibly ſhoved (by means of a ſtrong ſcrew) againſt the bottom of the

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[*] For fear I ſhould be ſuſpected of *prodigality* in the proſecution of my philoſophical reſearches, I think it neceſſary to inform the Society, that the cannon I made uſe of in this Experiment was not ſacrificed to it. The ſhort hollow cylinder which was formed at the end of it, was turned out of a cylindrical maſs of metal, about 2 feet in length, projecting beyond the muzzle of the gun, called in the German language the *verlorner kopf*, (the head of the cannon to be thrown away), and which is repreſented in Fig. 1.

This original projection, which is cut off before the gun is bored, is always caſt with it, in order that, by means of the preſſure of its weight on the metal in the lower part of the mould, during the time it is cooling, the gun may be the more compact in the neighbourhood of the muzzle; where, without this precaution, the metal would be apt to be porous, or full of honeycombs.

the bore of the cylinder, that the preſſure againſt it was equal to the weight of about 10000lb. Avoirdupois, the cylinder was turned round on its axis (by the force of horſes) at the rate of about 32 times in a minute.

This machinery, as it was put together for the Experiment, is repreſented by Fig. 2. W is a ſtrong horizontal iron bar, connected with proper machinery carried round by horſes, by means of which the cannon was made to turn round its axis.

To prevent, as far as poſſible, the loſs of any part of the Heat that was generated in the Experiment, the cylinder was well covered up with a fit coating of thick and warm flannel, which was carefully wrapped round it, and defended it on every ſide from the cold air of the atmoſphere. This covering is not repreſented in the drawing of the apparatus, Fig. 2.

I ought to mention, that the borer was a flat piece of hardened ſteel, 0.63 of an inch thick, 4 inches long, and nearly as wide as the cavity of the bore of the cylinder, namely, $3\frac{1}{2}$ inches. Its corners were rounded off at its end, ſo as to make it fit the hollow bottom of the bore; and it was firmly faſtened to the iron bar (*m*) which kept it in its place. The area of the ſurface by which its end was in contact with the bottom of the bore of the cylinder was nearly $2\frac{1}{3}$ inches. This borer, which is diſtinguiſhed by the letter *n*, is repreſented in moſt of the figures.

At the beginning of the Experiment, the temperature of the air in the ſhade, as alſo that of the cylinder, was juſt 60° F.

At the end of 30 minutes, when the cylinder had made 960 revolutions about its axis, the horſes being ſtopped, a cylindrical mercurial thermometer, whoſe bulb was $\frac{32}{100}$ of an inch in diameter, and 3¼ inches in length, was introduced into the hole made to receive it, in the ſide of the cylinder, when the mercury roſe almoſt inſtantly to 130°.

Though the Heat could not be ſuppoſed to be quite equally diſtributed in every part of the cylinder, yet, as the length of the bulb of the thermometer was ſuch that it extended from the axis of the cylinder to near its ſurface, the Heat indicated by it could not be very different from that of the *mean temperature* of the cylinder; and it was on this account that a thermometer of that particular form was choſen for this Experiment.

To ſee how faſt the Heat eſcaped out of the cylinder, (in order to be able to make a probable conjecture reſpecting the quantity given off by it, during the time the Heat generated by the friction was accumulating,) the machinery ſtanding ſtill, I ſuffered the thermometer to remain in its place near three quarters of an hour, obſerving and noting down, at ſmall intervals of time, the height of the temperature indicated by it.

Thus, at the end of	The Heat, as ſhown by the thermometer, was
4 minutes	126°
after 5 minutes, always reckoning from the firſt obſervation,	125°
at the end of 7 minutes	123°
12 ———	120°
14 ———	119°
16 ———	118°
20 ———	116°
24 ———	115°
28 ———	114°
31 ———	113°
34 ———	112°
37½ ———	111°
and when 41 minutes had elapſed	110°

Having taken away the borer, I now removed the metallic duſt, or rather ſcaly matter, which had been detached from the bottom of the cylinder by the blunt ſteel borer, in this Experiment; and, having carefully weighed it, I found its weight to be 837 grains Troy.

Is it poſſible that the very conſiderable quantity of Heat that was produced in this Experiment (a quantity which actually raiſed the temperature of above 113lb. of gun-metal at leaſt 70 degrees of FAHRENHEIT's thermometer, and which, of courſe, would have been capable of melting 6½lb. of ice, or of cauſing near 5lb. of ice-cold water to boil) could have been furniſhed by ſo inconſiderable a quantity of metallic duſt? and this merely in conſequence of *a change* of its capacity for Heat?

As the weight of this duſt (837 grains Troy) amounted to no more than $\frac{1}{948}$th part of that of the cylinder, it muſt have loſt no leſs than 948 degrees of Heat, to have been able to have raiſed the temperature of the cylinder 1 degree; and conſequently it muſt have given off 66,360 degrees of Heat, to have produced the effects which were actually found to have been produced in the Experiment!

But, without inſiſting on the improbability of this ſuppoſition, we have only to recollect, that from the reſults of actual and deciſive Experiments, made for the expreſs purpoſe of aſcertaining that fact, the capacity for Heat, of the metal of which great guns are caſt, *is not ſenſibly changed* by being reduced to the form of metallic chips, in the operation of boring cannon; and there does not ſeem to be any reaſon to think that it can be much changed, if it be changed at all, in being reduced to much ſmaller pieces, by means of a borer that is leſs ſharp.

If the Heat, or any conſiderable part of it, were produced in conſequence of a change in the capacity for Heat of a part of the metal of the cylinder, as ſuch change could only be *ſuperficial*, the cylinder would by degrees be *exhauſted;* or the quantities of Heat produced, in any given ſhort ſpace of time, would be found to diminiſh gradually, in ſucceſſive Experiments. To find out if this really happened or not, I repeated the laſt-mentioned Experiment ſeveral times, with the utmoſt care; but I did not diſcover the ſmalleſt ſign of *exhauſtion* in the metal, notwithſtanding the large quantities of Heat actually given off.

Finding ſo much reaſon to conclude, that the Heat generated in theſe Experiments, or *excited*, as I would rather chooſe to expreſs it, was not furniſhed *at the expence of the latent Heat* or *combined caloric* of the metal, I puſhed my inquiries a ſtep farther, and endeavoured to find out whether *the air* did, or did not, contribute any thing in the generation of it.

Experiment, N° 2.

As the bore of the cylinder was cylindrical, and as the iron bar (*m*), to the end of which the blunt ſteel borer was fixed, was ſquare, the air had free acceſs to the inſide of the bore, and even to the bottom of it, where the friction took place by which the Heat was excited.

As neither the metallic chips produced in the ordinary courſe of the operation of boring braſs cannon, nor the finer ſcaly particles produced in the laſt-mentioned Experiments by the friction of the blunt borer, ſhowed any ſigns of *calcination*, I did not ſee how the air could poſſibly have been the cauſe of the Heat that was produced; but, in an inveſtigation of this kind, I thought that no pains ſhould be ſpared to clear away the rubbiſh, and leave the ſubject as naked and open to inſpection as poſſible.

In order, by one deciſive Experiment, to determine whether the air of the atmoſphere had any part, or not, in the generation of the Heat, I contrived to repeat the Experiment, under circumſtances in which *it was evidently impoſſible for it to produce any effect whatever.* By means of a piſton exactly fitted to the mouth of the bore of the cylinder, through the middle of which piſton the ſquare iron bar, to the end of which the blunt ſteel borer was fixed, paſſed in a ſquare hole made perfectly air-tight, the acceſs of the external air, to the inſide of the bore of the cylinder, was effectually prevented. (In Fig. 3. this piſton (*p*) is ſeen in its place; it is likewiſe ſhown in Fig. 7 and 8.)

I did not find, however, by this Experiment, that the excluſion of the air diminiſhed, in the ſmalleſt degree, the quantity of Heat excited by the friction.

There ſtill remained one doubt, which, though it appeared to me to be ſo ſlight as hardly to deſerve any attention, I was however deſirous to remove. The piſton which cloſed the mouth of the bore of the cylinder, in order that it might be air-tight, was fitted into it with ſo much nicety, by means of its collars of leather, and preſſed againſt it with ſo much force, that, notwithſtanding its being oiled, it occaſioned a conſiderable degree of friction, when the hollow cylinder was turned round its axis. Was not the Heat produced, or at leaſt ſome part of it, occaſioned by this friction of the piſton? and, as the external air had free acceſs to the extremity of the bore, where it came in contact with the piſton, is it not poſſible that this air may have had ſome ſhare in the generation of the Heat produced?

Experiment, N° 3.

A quadrangular oblong deal box, (ſee Fig. 4.) water-tight, 11½ Engliſh inches long, 9$\frac{4}{10}$ inches wide, and 9$\frac{6}{10}$ inches deep, (meaſured in the clear,) being provided, with holes or ſlits in the middle of each of its ends, juſt large enough to receive, the one, the ſquare iron rod to the end of which the blunt ſteel borer was faſtened, the other, the ſmall cylindrical neck which joined the hollow cylinder to the cannon; when this box (which was occaſionally cloſed above, by a wooden cover or lid moving on hinges) was put into its place; that is to ſay, when, by means of the two vertical openings or ſlits in its two ends, (the upper parts of which openings were occaſionally cloſed, by means of narrow pieces of wood ſliding in vertical grooves,) the box (*g*, *h*, *i*, *k*, Fig. 3.) was fixed to the machinery, in ſuch a manner that its bottom (*i*, *k*,) being in the plane of the horizon, its axis coincided with the axis of the hollow metallic cylinder; it is evident, from the deſcription, that the hollow metallic cylinder would occupy the middle of the box, without touching it on either ſide (as it is repreſented in Fig. 3.); and that, on pouring water into the box, and filling it to the brim, the cylinder would be completely covered, and ſurrounded on every ſide, by that fluid. And farther, as the box was held faſt by the ſtrong ſquare iron rod (*m*), which paſſed, in a *ſquare hole*, in the centre of one of its ends, (*a*, Fig. 4.) while the round or cylindrical neck, which joined the hollow cylinder to the end of the cannon, could turn round freely on its axis in the *round hole* in the centre of the other end of it, it is evident that the machinery could be put in motion, without the leaſt danger of forcing the box out of its place, throwing the water out of it, or deranging any part of the apparatus.

Every thing being ready, I proceeded to make the Experiment I had projected, in the following manner:

The hollow cylinder having been previouſly cleaned out, and the inſide of its bore wiped with a clean towel till it was quite dry, the ſquare iron bar, with the blunt ſteel borer fixed to the end of it, was put into its place; the mouth of the bore of the cylinder being cloſed at the ſame time, by means of the circular piſton, through the centre of which the iron bar paſſed.

This being done, the box was put in its place, and the joinings of the iron rod, and of the neck of the cylinder, with the two ends of the box, having been made water-tight, by means of collars of oiled leather, the box was filled with cold water, (*viz.* at the temperature of 60°,) and the machine was put in motion.

The reſult of this beautiful Experiment was very ſtriking, and the pleaſure it afforded me amply repaid me for all the trouble I had had, in contriving and arranging the complicated machinery uſed in making it.

The cylinder, revolving at the rate of about 32 times in a minute, had been in motion but a ſhort time, when I perceived, by putting my hand into the water, and touching the outſide of the cylinder, that Heat was generated; and it was not long before the water which ſurrounded the cylinder began to be ſenſibly warm.

At the end of 1 hour I found, by plunging a thermometer into the water in the box, (the quantity of which fluid amounted to 18. 77lb. Avoirdupois, or 2¼ wine gallons,) that its temperature had been raiſed no leſs than 47 degrees; being now 107° of FAHRENHEIT's ſcale.

When 30 minutes more had elaſped, or 1 hour and 30 minutes after the machinery had been put in motion, the Heat of the water in the box was 142°.

At the end of 2 hours, reckoning from the beginning of the experiment, the temperature of the water was found to be raiſed to 178°.

At 2 hours 20 minutes it was at 200°; and at 2 hours 30 minutes it ACTUALLY BOILED!

It would be difficult to deſcribe the ſurprize and aſtoniſhment expreſſed in the countenances of

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the by-ſtanders, on ſeeing ſo large a quantity of cold water heated, and actually made to boil, without any fire.

Though there was, in fact, nothing that could juſtly be conſidered as ſurpriſing in this event, yet I acknowledge fairly that it afforded me a degree of childiſh pleaſure, which, were I ambitious of the reputation of a *grave philoſopher*, I ought moſt certainly rather to hide than to diſcover.

The quantity of Heat excited and accumulated in this Experiment was very conſiderable; for, not only the water in the box, but alſo the box itſelf, (which weighed 15¼ lb.) and the hollow metallic cylinder, and that part of the iron bar which, being ſituated within the cavity of the box, was immerſed in the water, were heated 150 degrees of FAHRENHEIT's ſcale; viz. from 60° (which was the temperature of the water, and of the machinery, at the beginning of the Experiment) to 210°, the Heat of boiling water at Munich.

The total quantity of Heat generated may be eſtimated with ſome conſiderable degree of preciſion, as follows:

Of the Heat excited there appears to have been actually accumulated,	Quantity of ice-cold water which, with the given quantity of Heat, might have been heated 180 degrees, or made to boil. *In Avoirdupois weight.*
In the water contained in the wooden box, 18¾ lb. Avoirdupois, heated 150 degrees, namely, from 60° to 210° F. - -	lb. 15.2
In 113.13 lb. of gun-metal, (the hollow cylinder,) heated 150 degrees; and, as the capacity for heat of this metal is to that of water as 0.1100 to 1.0000, this quantity of Heat would have heated 12½ lb. of water the ſame number of degrees - -	10.37
In 36.75 cubic inches of iron, (being that part of the iron bar to which the borer was fixed which entered the box,) heated 150 degrees; which may be reckoned equal in capacity for Heat to 1.21 lb. of water* - - - - -	1.01
Total quantity of ice-cold water which, with the heat actually generated by friction, and accumulated in 2 hours and 30 minutes, might have been heated 180 degrees or made to boil - - -	26.58

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* No Eſtimate is here made of the Heat accumulated in the wooden box, nor of that which muſt have been diſperſed, and loſt during the Experiment.

From the knowledge of the *quantity* of Heat actually produced in the foregoing Experiment, and of the *time* in which it was generated, we are enabled to aſcertain *the celerity of its production*, and to determine how large a fire muſt have been, or how much fuel muſt have been conſumed, in order that, in burning equably, it ſhould have produced by combuſtion the ſame quantity of Heat in the ſame time.

In one of Dr. CRAWFORD's Experiments, (ſee his Treatiſe on Heat, p. 321,) 37 lb. 7 oz. Troy, $=181920$ grains of water, were heated $2\frac{1}{10}$ degrees of FAHRENHEIT's thermometer, with the Heat generated in the combuſtion of 26 grains of wax. This gives 382032 grains of water heated 1 degree with 26 grains of wax; or $14693\frac{14}{26}$ grains of water heated 1 degree, or $\frac{14693}{180}=81.631$ grains heated 180 degrees, with the Heat generated in the combuſtion of one grain of wax.

The quantity of ice-cold water which might have been heated 180 degrees, with the Heat generated by friction in the before-mentioned Experiment, was found to be 26.58 lb. Avoirdupois, $=188060$ grains; and, as 81.631 grains of ice-cold water require the Heat generated in the combuſtion of 1 grain of wax, to heat it 180 degrees, the former quantity of ice-cold water, namely 188060 grains, would require the combuſtion of no leſs than 2303.8 grains ($=4\frac{8}{10}$ oz. Troy) of wax, to heat it 180 degrees.

As the Experiment (N° 3) in which the given quantity of Heat was generated by friction, laſted 2 hours and 30 minutes, = 150 minutes, it is neceſſary, for the purpoſe of aſcertaining how many wax-candles of any given ſize muſt burn together, in order that in the combuſtion of them the given quantity of Heat may be generated in the given time, and conſequently *with the ſame celerity* as that with which the Heat was generated by friction in the experiment, that the ſize of the candles ſhould be determined, and the quantity of wax conſumed in a given time by each candle, in burning equably, ſhould be known.

Now I found by an Experiment, made on purpoſe to finiſh theſe computations, that when a good wax-candle, of a moderate ſize, ¾ of an inch in diameter, burns with a clear flame, juſt 49 grains of wax are conſumed in 30 minutes. Hence it appears, that 245 grains of wax would be conſumed by ſuch a candle in 150 minutes; and that, to burn the quantity of wax (=2303.8 grains) neceſſary to produce the quantity of Heat actually obtained by friction in the Experiment in queſtion, and in the given time, (150 minutes,) *nine candles*, burning at once, would not be ſufficient; for 9 multiplied into 245 (the number of grains conſumed by each candle in 150 minutes) amounts to no more than 2205 grains; whereas the quantity of wax neceſſary to be burnt, in order to produce the given quantity of Heat, was found to be 2303.8 grains.

From the reſult of theſe computations it appears, that the quantity of Heat produced equably, or in a continual ſtream, (if I may uſe that expreſſion,) by the friction of the blunt ſteel borer againſt the bottom of the hollow metallic cylinder, in the Experiment under conſideration, was *greater* than that produced equably in the combuſtion of *nine wax-candles*, each $\frac{3}{4}$ of an inch in diameter, all burning together, or at the ſame time, with clear bright flames.

As the machinery uſed in this Experiment could eaſily be carried round by the force of one horſe, (though, to render the work lighter, two horſes were actually employed in doing it,) theſe computations ſhow further how large a quantity of Heat might be produced, by proper mechanical contrivance, merely by the ſtrength of a horſe, without either fire, light, combuſtion, or chemical decompoſition; and, in a caſe of neceſſity, the Heat thus produced might be uſed in cooking victuals.

But no circumſtances can be imagined, in which this method of procuring Heat would not be diſadvantageous; for, more Heat might be obtained by uſing the fodder neceſſary for the ſupport of a horſe, as fuel.

As ſoon as the laſt-mentioned Experiment (N° 3.) was finiſhed, the water in the wooden box was let off, and the box removed; and the borer being taken out of the cylinder, the ſcaly metallic powder, which had been produced by the friction of the borer againſt the bottom of the cylinder, was collected, and, being carefully weighed, was found to weigh 4145 grains, or about 8⅔ oz. Troy.

As this quantity was produced in 2½ hours, this gives 824 grains for the quantity produced *in half an hour.*

In the firſt Experiment, which laſted only *half an hour*, the quantity produced was 837 grains.

In the Experiment N° 1. the quantity of Heat generated, in *half an hour*, was found to be equal to that which would be required to heat 5 lb. Avoirdupois of ice-cold water 180 degrees, or cauſe it to boil.

According to the reſult of the Experiment N° 3. the Heat generated in *half an hour* would have cauſed 5.31 lb. of ice-cold water to boil. But, in this laſt-mentioned Experiment, the Heat generated being more effectually confined, leſs of it was loſt; which accounts for the difference of the reſults of the two Experiments.

It remains for me to give an account of one Experiment more, which was made with this apparatus, I found by the Experiment N° 1. how much Heat was generated when the air had free acceſs to the metallic ſurfaces which were rubbed together. By the Experiment N° 2. I found that the quantity of Heat generated was not ſenſibly diminiſhed when the free acceſs of the air was prevented; and by the reſult of N° 3. it appeared that the generation of the Heat was not prevented, or retarded, by keeping the apparatus immerſed in water. But as, in this laſt-mentioned Experiment, the water, though it ſurrounded the hollow metallic cylinder on every ſide, externally, was not ſuffered to enter the cavity of its bore, (being prevented by the piſton,) and conſequently did not come into contact with the metallic ſurfaces where the Heat was generated; to ſee what effects would be produced by giving the water free acceſs to theſe ſurfaces, I now made the

Experiment, Nº 4.

The piſton which cloſed the end of the bore of the cylinder being removed, the blunt borer and the cylinder were once more put together; and the box being fixed in its place, and filled with water, the machinery was again put in motion.

There was nothing in the reſult of this Experiment that renders it neceſſary for me to be very particular in my account of it. Heat was generated, as in the former Experiments, and, to all appearance, quite as rapidly; and I have no doubt but the water in the box would have been brought to boil, had the Experiment been continued as long as the laſt. The only circumſtance that ſurpriſed me was, to find how little difference was occaſioned in the noiſe made by the borer in rubbing againſt the bottom of the bore of the cylinder, by filling the bore with water. This noiſe, which was very grating to the ear, and ſometimes almoſt inſupportable, was, as nearly as I could judge of it, quite as loud, and as diſagreeable, when the ſurfaces rubbed together were wet with water, as when they were in contact with air.

By meditating on the reſults of all theſe Experiments, we are naturally brought to that great queſtion which has ſo often been the ſubject of ſpeculation among philoſophers; namely,

What is Heat?—Is there any ſuch thing as an *igneous fluid?*—Is there any thing that can with propriety be called *caloric?*

We have ſeen that a very conſiderable quantity of Heat may be excited in the Friction of two metallic ſurfaces, and given off in a conſtant ſtream or flux, *in all directions*, without interruption or intermiſſion, and without any ſigns of *diminution* or *exhauſtion.*

From whence came the Heat which was continually given off in this manner, in the foregoing Experiments? Was it furniſhed by the ſmall particles of metal, detached from the larger ſolid maſſes, on their being rubbed together?—This, as we have already ſeen, could not poſſibly have been the caſe.

Was it furniſhed by the air?—This could not have been the caſe; for, in three of the Experiments, the machinery being kept immerſed in water, the acceſs of the air of the atmoſphere was completely prevented.

Was it furniſhed by the water which ſurrounded the machinery?—That this could not have been the caſe is evident: *firſt*, becauſe this water was continually *receiving Heat* from the machinery, and could not, at the ſame time, be *giving to*, and *receiving Heat from*, the ſame body; and *ſecondly*, becauſe there was no chemical decompoſition of any part of this water. Had any ſuch decompoſition taken place, (which indeed could not reaſonably have been expected,) one of its component elaſtic fluids (moſt probably inflammable air) muſt, at the ſame time, have been ſet at liberty, and, in making its eſcape into the atmoſphere, would have been detected; but though I frequently examined the water to ſee if any air bubbles roſe up through it, and had even made preparations for catching them, in order to examine them, if any ſhould appear, I could perceive none; nor was there any ſign of decompoſition of any kind whatever, or other chemical proceſs, going on in the water.

Is it poſſible that the Heat could have been ſupplied by means of the iron bar to the end of which the blunt ſteel borer was fixed? or by the ſmall neck of gun-metal by which the hollow cylinder was united to the cannon?—Theſe ſuppoſitions appear more improbable even than either of thoſe before mentioned; for Heat was continually going off, or *out of the machinery*, by both theſe paſſages, during the whole time the Experiment laſted.

And, in reaſoning on this ſubject, we muſt not forget to conſider *that moſt remarkable circumſtance*, that the ſource of the Heat generated by friction, in theſe Experiments, appeared evidently to be *inexhauſtible*.

It is hardly neceſſary to add, that any thing which any *inſulated* body, or ſyſtem of bodies, can continue to furniſh *without limitation*, cannot poſſibly be *a material ſubſtance*: and it appears to me to be extremely difficult, if not quite impoſſible, to form any diſtinct idea of any thing, capable of being excited and communicated, in the manner the Heat was excited and communicated in theſe Experiments, except it be MOTION.

I am very far from pretending to know how, or by what means, or mechanical contrivance, that particular kind of motion in bodies, which has been ſuppoſed to conſtitute Heat, is excited, continued, and propagated; and I ſhall not preſume to trouble the Society with mere conjectures; particularly on a ſubject, which, during ſo many thouſand years, the moſt enlightened philoſophers have endeavoured, but in vain, to comprehend.

But, although the mechaniſm of Heat ſhould, in fact, be one of thoſe myſteries of nature which are beyond the reach of human intelligence, this ought by no means to diſcourage us, or even leſſen our ardour, in our attempts to inveſtigate the laws of its operations. How far can we advance in any of the paths which ſcience has opened to us, before we find ourſelves enveloped in thoſe thick miſts which, on every ſide, bound the horizon of the human intellect? But how ample, and how intereſting, is the field that is given us to explore!

Nobody, ſurely, in his ſober ſenſes, has ever pretended to underſtand the mechaniſm of gravitation; and yet what ſublime diſcoveries was our immortal NEWTON enabled to make, merely by the inveſtigation of the laws of its action!

The effects produced in the world by the agency of Heat are probably *juſt as extenſive*, and quite as important, as thoſe which are owing to the tendency of the particles of matter towards each other; and there is no doubt but its operations are, in all caſes, determined by laws equally immutable.

Before I finiſh this Eſſay, I would beg leave to obſerve, that although, in treating the ſubject I have endeavoured to inveſtigate, I have made no mention of the names of thoſe who have gone over the ſame ground before me, nor of the ſucceſs of their labours; this omiſſion has not been owing to any want of reſpect for my predeceſſors, but was merely to avoid prolixity, and to be more at liberty to purſue, without interruption, the natural train of my own ideas.

DESCRIP-

Fig. 1.

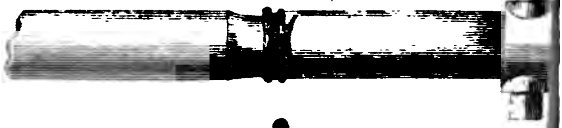


Fig. 2.

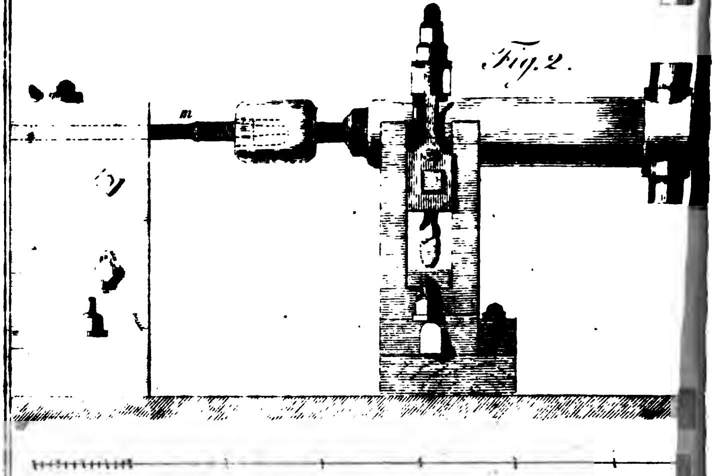
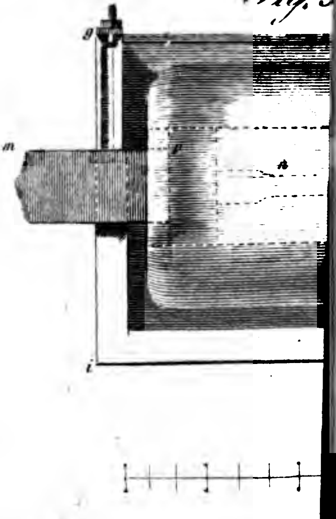


Fig. 3



DESCRIPTION *of the* FIGURES.

Fig. 1. ſhows the cannon uſed in the foregoing xperiments, in the ſtate it was in when it came om the foundry.

Fig. 2. ſhows the machinery uſed in the Ex-eriments N° 1. and N° 2. The cannon is ſeen xed in the machine uſed for boring cannon. W s a ſtrong iron bar, (which, to ſave room in the lrawing, is repreſented as broken off,) which bar, eing united with machinery (not expreſſed in the igure) that is carried round by horſes, cauſes the cannon to turn round its axis.

m is a ſtrong iron bar, to the end of which the blunt borer is fixed; which, by being forced againſt the bottom of the bore of the ſhort hollow cylinder that remains connected by a ſmall cylindrical neck to the end of the cannon, is uſed in generating Heat by friction.

Fig. 3. ſhows, on an enlarged ſcale, the ſame hollow cylinder that is repreſented on a ſmaller ſcale in the foregoing Figure. It is here ſeen connected with the wooden box (*g*, *h*, *i*, *k*,) uſed in the Experiments N° 3. and N° 4. when this hollow cylinder was immerſed in water.

p, which is marked by dotted lines, is the piſton which cloſed the end of the bore of the cylinder.

n is the blunt borer ſeen ſidewiſe.

d, *e*, is the ſmall hole by which the thermometer was introduced, that was uſed for aſcertaining the Heat of the cylinder. To ſave room in the drawing, the cannon is repreſented broken off near its muzzle; and the iron bar, to which the blunt borer is fixed, is repreſented broken off at *m*.

Fig. 4. is a perſpective view of the wooden box, a ſection of which is ſeen in the foregoing Figure. (See *g*, *h*, *i*, *k*, Fig. 3.)

Fig. 5. and 6. repreſent the blunt borer *n*, joined to the iron bar *m*, to which it was faſtened.

Fig. 7. and 8. repreſent the ſame borer, with its iron bar, together with the piſton, which, in the Experiments N° 2. and N° 3. was uſed to cloſe the mouth of the hollow cylinder.

END OF THE SECOND VOLUME.

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